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ART. XXXVI.—*On some of the ancient Glaciers of the Sierras;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist. Univ. of California.* With a Map on Plate V.

DURING the past summer, in company with several young men, mostly students and graduates of the University of California, I spent some four or five weeks in camp, among the high Sierras, examining the traces of the ancient glaciers of that region. Two years ago I carried a similar party over nearly the same ground. In addition to what has already been made known by the Geological Survey, I observed many phenomena which I think cannot fail to interest geologists and physicists.

That I may be clearly understood, I will very briefly describe my route.† From Oakland I went immediately to Yosemite, and spent about a week in that valley examining the evidences of glacial occupation. In this examination I was greatly assisted, in fact my observations were almost entirely directed, by Mr. John Muir, a gentleman of rare power of scientific observation, who has lived several years in the valley and devoted much time to glacial studies, and who undoubtedly knows more about the ancient glaciers in the vicinity of Yosemite than any man living. After satisfying myself that this magnificent valley, if not *formed*, was certainly *once occupied*, by a great glacier, I went up the Nevada cañon to the top of Nevada falls, and thence by the Mono trail (dotted line on map) along the margin of Little Yosemite Cañon, then up Feldspar valley (F. V.), by

* Read before the Cal. Acad. of Science, Sept. 16, 1872.

† The map accompanying this paper (Plate V) is reduced from a part of the excellent chart by Hoffmann and Gardner, issued by J. D. Whitney in connection with the publication of the California Geological Survey under his charge.

the base of Cathedral Peak, to the Tuolumne Meadows at Soda Springs. Here we left the Mono trail and went up the Tuolumne meadows to the foot of Mt. Lyell, and climbed this mountain. From Mt. Lyell we returned to the Mono trail, and followed it over Mono pass, down Bloody Cañon to Lake Mono and the extinct volcanoes in that vicinity. From Lake Mono we went northward, among the mountain valleys on the east side of the crest, to Lake Tahoe; and then by Johnson's pass and the Placerville road to Placerville and Sacramento and so back to Oakland. From the Yosemite to Mt. Lyell I was accompanied by Mr. Muir and Mr. Galen Clark, both men of rare intelligence and accomplished mountaineers.

During my previous visit the route was a little different. From Yosemite to Soda Springs I took the Coulterville Mono trail, which passes by Mt. Hoffman and Lake Tenaya. On that occasion too I ascended Mt. Dana. The whole distance (near 600 miles) in both cases was accomplished on horseback. I had, therefore, good opportunity and leisure for observing. I found abundant evidences of ancient glaciers in almost every part of the Sierra. Among those observed I will select a few of the most interesting.

I. The Yosemite or Merced Glacier.

There can be no doubt, I think, that the Yosemite valley was once filled to its *brim* by a great glacier. Whitney, in his Geological Survey, distinctly states his belief that a glacier once occupied this valley to the depth of 1000 feet; this belief being mainly founded upon the supposed remains of old moraines discovered by Mr. King.* But this opinion he subsequently retracted in his *Yosemite Guide Book*,† and now thinks there is no sufficient evidence of the former existence of such a glacier. In my opinion his first conclusion is undoubtedly the correct one.

It is true that *glaciated surfaces* are not made out with certainty in the main valley, and the *terminal moraines* are not so clear as might be desired; but glaciated *forms* are unmistakably observable on the walls of the valley in many places, and in some places even to the brim.

In order to understand why there has been so much doubt as to the former existence of glaciers in the lower valleys of the Sierras, it must be remembered in the first place that the granite in this region decomposes very rapidly; and in the second place that an immense period of time separates the epoch of the great glaciers such as that of which we are now speaking, and that of the smaller glaciers which occupied only the upper portion of the higher valleys. It is in these upper valleys only that glaciated surfaces are very conspicuous. And yet even

* Vol. 1, p. 422 and seq. † p. 73.

there, these surfaces, although so comparatively recent, are only found in patches. A few hundred or thousand years will probably remove every vestige of them, except where protected by soil. Glaciated *forms*, on the contrary, are much more permanent; but these are most conspicuous always in the bed of the glacier; and there they are apt to be covered up, after the retreat of the glacier, by lake deposits and by debris brought by tributary glaciers coming down through side cañons. In the Yosemite the glacial bed rock has been entirely covered up by these means, particularly by *lake deposits*, which are here very conspicuous. So far as glaciated *forms* and surfaces are concerned, therefore, only the walls remain to tell the story.

Mr. Muir first drew my attention to the fact that the almost perpendicular walls of Yosemite are in many places absolutely *free from talus*, the hard, smooth rocky wall coming down directly to the level meadow soil of the valley. In every such place the wall rock is found to be very hard and solid, more or less projecting and rounded, with a gentle slope on the side looking up the valley, and with a more abrupt curve or even broken on the lower side; in other words it is a true *moutonnée* form. Other parts of the wall are irregular and destitute of glacial signs; but the reason is evident in the piles of debris at the bottom. The former consist of hard material, little affected by joints, and have, therefore, remained in the condition in which the glacier left them; the latter consist of softer materials, and have crumbled and thus destroyed the glacial signs since the period of glacial occupation. Thus, on the north side of the valley every projecting shoulder is rounded and smooth and without talus, while every re-entering angle is rough and jagged, and has its pile of debris at its base. In some cases, as for example high up on Washington column, the smoothness of these projecting shoulders is such that they glisten in the sunshine. I believe if these spots could be reached they would be found scratched and polished.

These *moutonnée* forms are found principally on the north wall. The reason is evident. In an east and west chasm so deep, so narrow, and with walls so perpendicular as this, the sun shines with full effect only on the northern wall. Therefore ice and snow and small tributary glaciers hung about the cliffs and side-cañons, on the south side, long after the retreat of the main glacier, and after the northern wall was entirely clear. Under the disintegrating influence of this frost, the glaciated forms and surfaces produced by the main glacier have been entirely destroyed on the southern wall.

Again: although the old terminal moranies are not so clear as might be desired, yet I think no one who was examined the glacial valleys of the Sierras, and thereby made himself thoroughly familiar with the appearance of half-effaced moraines,

can fail to perceive them in Yosemite. The bottom of this valley, as of all the glacial valleys of the Sierras, consists of a succession of level meadows covered with grass and flowers, separated by higher ground overgrown with pines. The soil of the meadows is *stratified sands and gravels*, while that of the higher ground is composed of boulders of all sizes mixed with finer material without sorting—in other words, of *moraine matter*. These boulder piles are certainly terminal moraines, and the meadows are ancient lakes or marshes, produced by the damming up of the river waters by successive terminal moraines left by the retreat of the glacier and afterward filled up by river silt.

Tributaries of Merced Glacier.—The north fork of the Merced river now drains the whole area bounded by Mt. Hoffman, Cathedral Peak, Mt. Lyell and Mt. Clark. (See map.) Its tributaries are Yosemite creek coming from Mt. Hoffman, Tenaya creek from Cathedral Peak, and other peaks in the vicinity, the main Merced coming from Mt. Lyell group, and Illiluette from Mt. Clark group. In glacial times precisely the same basin was drained by the *Merced glacier*, and there was a tributary *glacier* corresponding to each of these tributary *rivers*. Only two of these I have personally examined. Mr. Muir has examined them all.

(a.) *Little Yosemite Glacier.*—The most important of these tributaries, in fact the main feeder of the Merced glacier, was formed by ice streams coming from Cathedral peak, and especially from Mt. Lyell group, which, uniting in Little Yosemite valley, passed down as ice cascades over the Nevada falls and the Vernal falls, and so joined the other tributaries, especially the Tenaya glacier, to form the great glacier which filled Yosemite. At the point of former junction with the Tenaya, as first pointed out by Mr. King, a median moraine is still traceable.

Our route, as already stated, passed up the Nevada cañon to the top of the Nevada falls; thence along the rim of Little Yosemite valley, and thence over a ridge into Feldspar valley (F. V.), and up this valley to Cathedral peak. Glaciated surfaces and erratics were found nearly all the way. As we passed along the edge of the rim of Little Yosemite, we had a magnificent bird's-eye view of the wonderful dome-like form of nearly all the prominent points about this valley. Standing thus above, and looking down upon them, their striking resemblances to *glaciated forms* cannot be overlooked. The whole surface of the country is *moutonnée* on a huge scale. If so, then the greater domes about the Yosemite have been formed in a similar manner. If so, then the whole surface of this region, with its greater and smaller domes, has been moulded beneath an universal ice-sheet, or confluent glacier which moved onward with a steady

current careless of domes. The period of this ice-sheet, of course, preceded that of the separate glaciers which I am now tracing.

Another point of some interest which I observed here was the existence, everywhere in the lower part of Little Yosemite, even down to the Nevada falls, of erratics of a peculiar granite composed of very large and very perfect crystals of feldspar, cemented together by a paste of finer granite. These crystals, which were often four or five inches in length, stood out on the weathered surfaces so that the boulders looked like masses of coarse conglomerate or breccia. We traced these boulders up Feldspar valley (so-called from this remarkable granite) to their parent rock, viz: Cathedral peak and other peaks and comb-like ridges in that vicinity.

Cathedral Peak (11,000 feet high) is the most conspicuous of a cluster of sharp spire-like peaks and comb-like ridges in this vicinity; spires and ridges so steep and sharp that it seems difficult to understand how they can remain standing from year to year. These seem all to be formed of the peculiar granite spoken of. From the immense masses of snow which once accumulated amongst these peaks, and which still linger there in small quantities in sheltered spots, in glacial times there issued a glacier which passed down Feldspar valley and, joining the Little Yosemite glacier, poured its icy flood over the Nevada and Vernal falls into the Yosemite valley.

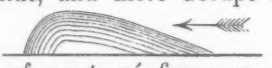
(b.) *Tenaya Glacier.*—From the same group of peaks emerged another glacier, which flowed farther west into the valley of the Tenaya fork. This tributary glacier was there joined by a stream of ice coming from the overflow of the Tuolumne glacier at D, as already pointed out by Whitney; and the combined streams flowed down the Tenaya cañon into the Yosemite valley. In the upper track of this glacier, especially of the tributary which emerged from the Cathedral peaks, I found the most beautiful examples of glaciated surfaces I have ever seen; rock basins scooped out by the glacier and now filled with pure water, forming exquisite little lakes; acres of glaciated surfaces, so smooth that it was difficult to ride over them. These glaciated surfaces were the more beautiful on account of the large feldspar crystals contained in the granite. A glaciated surface had the appearance of polished brecciated marble. Just above Lake Tenaya stands a mass of granite 800 feet high and half a mile or more long, directly in the track of the glacier. It has been polished on every side and over the top until it is rounded like the carapace of a tortoise.

The Tenaya cañon between Lake Tenaya and Mirror lake I have not examined. Just below Mirror lake there is a pile of boulders across the cañon. This I believe to be a moraine.

Below this and before reaching Yosemite proper, there is a succession of small meadows, three in number, separated by debris piles. These have been formed, I think, in the same manner as Mirror lake, viz., by water accumulated behind moraines left by the retreating glacier.

II. Tuolumne Glacier.

Whitney has already pointed out the fact, that at one time a great glacier filled the Tuolumne meadows, and after overflowing its banks at the divide between this valley and the basin of the Merced, and sending a branch down the Tenaya cañon, as already explained, passed down the course of the Tuolumne. One of the moraines left by its subsequent retreat is seen a few miles below Soda Springs. This glacier, according to Whitney, probably passed into, filled and passed beyond, Hetch-hetchy valley. Traced from its source in Mt. Lyell to the mouth of the Hetch-hetchy valley, this glacier could not have been less than forty miles long.

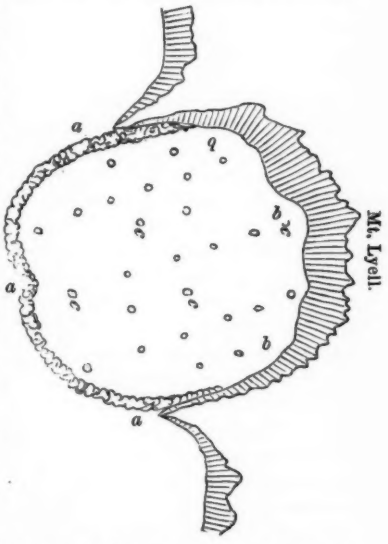
Tributaries.—The great Tuolumne glacier had many tributaries, three of which I have personally examined. One of these may be traced by a succession of meadows and other glacial signs up to Mt. Dana and Mt. Gibbs, a second in a similar manner to Mono pass, while the main branch is very clearly traceable up the Tuolumne meadows to Mt. Lyell and Mt. McClure. All of these tributaries met above Soda Springs. At this point, directly in the path of the combined glacier, stand two rocky knobs 500 to 800 feet high. They are smoothed and rounded on every side and over top, more sloping on the upper or eastern side, and more abrupt on the lower or western side, thus:  In a word they are perfect examples of *moutonnée* forms on a grand scale. But what interested me far more than anything else, was that the main branch of the Tuolumne glacier, far up among the cliffs and peaks of Mt. Lyell, *still exists as a living glacier*, in a feeble state of vitality it is true, but certainly living. My attention was first directed to this fact by Mr. Muir,* and I visited Mt. Lyell with him for the purpose of convincing myself of so interesting a fact.

The summits of Mt. Lyell (13,300 feet high) and Mt. McClure consist of a vast amphitheater, irregularly circular in form and several miles in diameter, surrounded by almost perpendicular cliffs with sharp jagged crests. In this circular amphitheater there still exist extensive snow-fields, no doubt of considerable thickness. In glacial times this great amphitheater was completely filled to the brim with snow. It was the womb in

* See this Journal, for January, vol. v, p. 69.

which originated and from which emerged the Tuolumne glacier. From the immense mass of snow accumulated here, the glacier poured its icy flood down the Tuolumne meadows to Soda Springs, where, joined by other tributaries, the swollen stream overflowed its banks and poured a portion of its flood over the divide into the Tenaya cañon, while the greater portion went down the Tuolumne valley to, and beyond Hetch-hetchy; how far is not known. As the Glacial period passed away, the point of the Tuolumne glacier receded again, leaving terminal moraines here and there in the course of its retreat. When the point passed again the place of confluence above Soda Springs, the tributaries separated and retreated each its several way to the snow fields above. This retreat has been already completed; the points of the glaciers have been retired within the snow fields; these snow fields consist wholly of snow and névé and not, as far as can be seen, of glacier-ice. Nevertheless, in the case of the snow-remnant of the main branch of the Tuolumne glacier in the Mt. Lyell amphitheater, *true glacial motion still continues.*

The evidences of this important fact are abundant. The whole irregular amphitheater formed by Mt. Lyell, Mt. McClure and other unnamed summits, is not now filled with snow, but only the more sheltered *coves.* One of these, viz., that formed by Mt. Lyell, I have rudely represented in the accompanying figure. The mass of snow occupying this cove is about a mile in length, and about half a mile wide. Now, along the lower margin of this snow field and closely in contact with it, there is as perfect a *terminal moraine* as can be imagined. It is an irregularly crescentic line of boulders *a a a*, forming a pile 20 feet high, 50 feet wide at base, and about a mile long. On the surface of the snow also may be seen scattered blocks of stone *c c c*, some of them of great size, which have fallen from the perpendicular cliffs *b b b*, and are on their way to join the terminal moraine below. Some of these could be traced in lines to points of the cliff which have contributed in greater abundance.



Between the cliff *b b b* and the snow, there is an empty space like a crevasse, 4 to 5 feet wide, evidently produced by the tearing away of the moving snow from the perpendicular cliffs. In the language of Alpine travelers, it is a *bergschrund.* Mr. Muir has also found a crevasse several feet wide in a similar snow field on Mt. McClure.

After examining the snow field of Mt. Lyell, our party left for Mono lake. Mr. Muir and Mr. Clarke remained, with the intention of re-ascending either Mt. Lyell, or Mt. McClure, and driving stakes in a line across the snow for the purpose of determining the rate of motion by subsequent examination. After my return to Oakland I learned from Mr. Muir the result of these experiments. When examined again at the end of 46 days, stake No. 1 had moved 11 inches, No. 2 18 inches, No. 3 34 inches, and No. 4 which was near the middle, 47 inches.* Both the fact of *motion* and its *differential character* are therefore certain.

Here, then, on Mt. Lyell we have *now existing, not a true glacier* perhaps, certainly *not a typical glacier* (since there is no true glacier-ice *visible*, but only snow and névé; and certainly *no protrusion of an icy tongue beyond the snow field*); yet, nevertheless, *in some sense a glacier*, since there is true differential motion and a well-marked terminal moraine. It is in fact a glacier in its feeble old age—feeble remnants of the great Tuolumne glacier—a glacier once of grand proportions and playing an important part in mountain sculpture, but now in its *second childhood.*

Mr. Muir tells me that he has found, hiding away among the highest peaks of the Sierras, several elderly glaciers in a similar sad condition.

Before leaving the subject of Mt. Lyell glacier (if I might so call it), there is another point to which I would call the attention of physicists. The surface of the snow of this great snow field I found traversed, in a direction at right angles to the slope, by *sharp blades* of half compacted ice, about two feet apart and two, three, four or even five feet high, so that a section in the direction of slope would be somewhat like the adjoining figure. Climbing up the slope over these ice blades was certainly the most difficult climbing I have ever attempted. Stepping from blade to blade, was attended with great risk of repeated

* An account of these experiments, taken from the Overland Monthly, will be found in this Journal for Jan., vol. v, p. 69.

and painful falls. The crests of these blades were not continuous, but irregular both in outline and in trend; very much in this respect like ripple-marks or like waves. Again, the irregularities in successive blades or waves were so related to each other, as to give rise to lighter *lines of slope* distinctly visible at a distance. Thus viewed the two sets of lines were related to each other like the lines of an engraved map, the blades representing the *level lines*, and the lighter lines the lines of slope, so that a surveyor had only to copy these in order to make a perfect map.

I was for some time at a loss to account for these blades. I at first thought they were in some way connected with the motion of the glacier, and might be transverse crevasses sharpened by action of the sun. But I now believe that the true explanation was given me a few weeks ago, in conversation, by Mr. J. T. Gardiner, formerly of the Geological Survey. According to Mr. Gardiner, the transverse ridges or blades are produced by the *action of sun on wind ripples.* During the winter the wind blows mainly down the cañon and the loose snow is drifted into wind-ripples; during the summer, when neither rain nor snow falls for many months, the snow is greatly wasted, but more in the troughs than on the crests, on account of the reverberation of heat within the troughs. The lighter lines were, of course, in the direction of the wind. Whether or not *sand ripples* produced by wind or by currents exhibit also lighter lines in the direction of the current, I do not know.

III. Bloody Cañon Glacier.

The Sierras on their western side slope gradually for 50 or 60 miles; but on the eastern side they are very precipitous, so that the plains, 5000 to 7000 feet below the crests, are reached in two or three miles. In glacial times, long complicated glaciers, with many tributaries, occupied the western slope, while on the eastern slope innumerable short, simple glaciers flowed in parallel streams down the steep incline and out, for several miles, on the level plain. The only one I have carefully examined is that which occupied Bloody Cañon. This glacier has been mentioned by Whitney, but there are several points of interest which are not mentioned in his very brief sketch.

Mono pass is a watershed from which runs a stream in either direction. In glacial times it was a *snowshed*, from which issued a *glacier* in either direction. One of these flowed westward, and formed a tributary of Tuolumne glacier, as already explained, while the other flowed eastward down the steep incline of Bloody Cañon to the level plains of Mono and out on these plains for several miles. I know no place where the formation of glacial lakes can be so well studied.

Bloody Cañon descends about 4000 feet in two or three miles. The glaciation of the walls and bottom of this steep, narrow, rocky gorge is extremely beautiful. About one-third of the way down, just where the steep declivity becomes nearly level, only to plunge downward again, occurs an exquisitely beautiful lake. Its borders, especially on the upper and lower sides, are polished and scored in the most perfect manner, and its bottom so far as could be seen is also smooth and rocky. Without doubt this is a *pure rock basin scooped out by the glacier* at this place, on account of the change of inclination here.

When the tourist has reached the level plains, after he has escaped from the rocky walls of the cañon, he still finds himself confined between two *parallel ridges*, composed entirely of boulders and earth, at least 500 feet high, only half a mile apart, and extending five or six miles out on the plains. These parallel ridges are evidently the *huge lateral moraines of Bloody Cañon glacier*. Just where the level plain, and, therefore, the debris ridges, commence, there is a lake of considerable size, occupying nearly the whole space between the ridges. Immediately below this lake, the ridges are seen to send off each a branch ridge, which, curving toward each other, meet in the middle. This crescentic transverse ridge connecting with the main lateral ridges is evidently a terminal moraine, behind which the lake has accumulated. Through this, of course, the stream has cut its way. Beyond this terminal moraine there is a *marsh*. Below this the lateral moraines are again seen to send off each a branch which, curving, again meet. It is another terminal moraine, behind which was probably formed first a shallow lake, which has been gradually filled up with sediment and converted into a marsh. Beyond the marsh is a *meadow* which has been evidently formed in the same way, for below the meadow there are evidences of still another terminal moraine.

From the summit of any of the volcanoes near by on the plains, a fine bird's-eye view of all I have described is obtained. The long lateral moraines and the three or more terminal moraines separating the lake, marsh, and meadow, are seen at one glance. In addition, many other *similar* ridges are seen stretching out on the plains from the gorges of the mountains, some of far greater height and length than those described. They are all no doubt moraines, but I have only observed them at a distance.

Whitney, in the first volume of the Geological Survey of California (p. 437), speaks of the lakes in Bloody Cañon, but ascribes them all to the collection of water behind terminal moraines. But it is evident that the *upper one* is a pure rock basin; its lower rim is not a terminal moraine, but is a beautifully

glaciated rock. In fact, we have here admirable examples of the two kinds of glacial lakes; those of one kind are accumulated in *rock basins* scooped out by glacial erosion; those of the other kind are accumulated behind terminal moraines; the former are found only in the higher slopes, the latter are usually found lower down. These latter graduate insensibly into marshes and meadows. Standing on the top of Mt. Dana on my previous trip, I am sure more than 50 of these lakes and marshes could be counted.

After leaving Mono lake, our party traveled northward among the foothills on the east side of the crests of the Sierras. We were too far from the crests to observe any glaciated surfaces or glaciated forms. Until we approached Lake Tahoe, I saw no unmistakable signs of glacial action, except here and there what seemed to be long hills of debris brought down from the Sierras. Nevertheless, the sharp, jagged, saw-like crests of the Sierras seen in the distance, with their palisade-like cliffs and circular amphitheaters still filled with snow, clearly showed that these were the deserted homes of many former glaciers.

IV. Carson Cañon Glacier.

Hope valley and Lake valley (Lake Tahoe valley) lie countersunk upon the very top of the Sierras and in the direction of the chain. This chain, therefore, is divided at this point into *two crests*, east and west of these valleys. From the plains of Carson, on the east side of the Sierras, our party passed westward through Carson Cañon, a magnificent gorge which cuts through the eastern crest into Hope valley. Throughout this cañon I observed the clearest evidences of glacial action, in the form of scored, polished and *moutonnée* surfaces. In Hope valley the same evidences were abundant. From the direction of the scratches, it was evident that a glacier once came from the south down Hope valley, and turned at right angles and passed out by Carson Cañon. Looking southward from Hope valley, a cluster of snowy peaks was distinctly visible about 15 or 20 miles distant; from these no doubt came the glacier. I did not trace this glacier to its source; but Mr. Hawkins, a student of the University and one of our party, who lives in this vicinity, and has been thoroughly educated in the observation of glacial signs, subsequently went up this and other valleys to the southward and examined them carefully. To the south of Hope valley is Faith valley, and to the south of this Charity valley These three are separated from each other only by low rocky ridges. These ridges are polished, scored and moutonnéed in the most perfect manner; they are, in fact, the lips of consecutive lake basins scooped out by a glacier.

It is perfectly evident from the direction of the scorings observed by myself and by Mr. Hawkins, that a great glacier came down from the snowy peaks to the southward, and, gathering tributaries from the peaks on either side but especially from the eastern crest, filled the whole basin of the three valleys, forming an immense *mer de glace* 15 miles long and 3 or 4 miles wide, which, being blocked to the northward by high mountains, themselves probably contributing their share of ice, turned at right angles and escaped eastward to the plains of Carson, by the deep narrow gorge of Carson cañon.

V. Lake Valley Glacier.

From Hope valley our party passed westward over a low ridge of debris, then through a cross valley nearly filled with a shallow lake and its surrounding marsh and meadow, and then down into Lake valley. The ridge of debris is about 600 feet above Hope valley, and 100 feet above the cross valley. Although I could not find the clear evidences of glaciated surfaces, yet I feel almost sure that the Hope valley *mer de glace* found a second outlet *to the west* through this cross valley into Lake valley. The debris pile spoken of is, I think, a lateral moraine of Hope valley glacier, formed at a later period, when this higher outlet had dried up.

Lake valley heads apparently in the same region of snowy summits as Hope valley. The two valleys run nearly parallel to each other, but Lake valley extends much farther northward, and is lower by several hundred feet. In this valley I found again evidences, which I could not misunderstand, of the former presence of a glacier. The glaciated surfaces were not so clear as in Hope valley and Carson cañon, but the rounded forms of the nearly perpendicular projecting rocky shoulders on each side of the valley were unmistakably due to ice. I have no doubt that well marked glaciated surfaces would be found in abundance higher up the valley, or by diligent search even in the region I passed over. Evidences of the main glacier, however, became less and less clear, as we passed down toward Lake Tahoe; being obscured partly by the more recent action of tributary glaciers coming down the side cañons, and partly, as we approached the lake, by what I take to be lake deposits, indicating a former higher and much more extensive condition of the lake. I conclude, from what I observed, that at one time a great glacier came down from the south, filling the whole Lake valley for 20 miles, then filling the great basin of Lake Tahoe 25 miles long, 15 miles wide, and 1600 feet deep, and finally escaped northward and eastward by the Truckee cañon. Both Lake valley and Lake Tahoe are flanked on either side by high mountains: this great *mer de glace* therefore received tributaries at every step.

VI. American River Glacier.

On returning homeward we retraced our steps southward up Lake valley for about 8 miles, then crossed the western crest of the Sierras by Johnson's pass, and then went down the cañon of the south fork of the American river.

Among the tributaries which flowed into Lake valley glacier, one of the largest came down from Johnson's pass. The scored, polished, *moutonnéed* surfaces were very conspicuous and beautiful. From the same pass another much greater glacier flowed westward and southward down the grand cañon of the south fork of the American river, for at least 25 miles. For this distance glaciated surfaces and glaciated forms are distinct, and in many places very fine. For example, at Sugar Loaf, about 20 miles down from the pass, the gorge becomes very narrow and precipitous, with perpendicular rock walls which almost rival in grandeur the cliffs of Yosemite. Through this narrow gorge the glacier was squeezed. The effects of glacial erosion are seen on the sides, but especially on several rocky prominences which stand in the middle of the gorge. These have been smoothed and scored on every side in the most perfect manner. One of these is the "*Sugar-loaf rock*," from which the village takes its name.

Four or five miles below this point, glacial marks disappear, and the cañon changes its character. The upper part of the cañon consists of a succession of broad level *meadows;* the lower part is sharply V-shaped, and contains no longer any meadows. The change marks, I believe, the distinction between *glacial* and *river erosion.* This change, however, marks also the change from granite to slate. Glaciers may have passed still lower down the cañon; but if so the glacial form of the cañon has been greatly modified by water-erosion, which has cut far below the glacial bed.

VII. Some general Reflections.

A. *General Ice-sheet in California.*—It is generally admitted that during the glacial epoch the polar ice-cap extended southward in North America, at least as far as 40° N. lat. This universal ice-sheet flowed southward and eastward on the Atlantic slope—southward in the middle region of the continent, and probably southward and westward on the Pacific slope, in Oregon, and Washington territory: in other words, its general direction was southward and seaward. We have given 40° as about the southern margin; but from this southern margin the general ice-sheet stretched out finger-like prolongations still farther south down the valleys, in the form of *separate* glaciers. The position of many of these separate glaciers have been traced by eastern observers. There can be no donbt, however,

that, favored by the great altitude of the Sierras, the universal ice-sheet itself was extended along this chain at least to Middle California, lat. 37°, and probably even to Southern California, lat. 33° or 34°. The flow of the ice-sheet in the Sierras was probably determined by the slope of this great chain, rather than by the arctic elevation; it was eastward and westward from the crest, rather than southward along the range. This general ice-sheet stretched out finger-like projections eastward and westward in the form of separate glaciers, far down the lower valleys.

The former existence of such a flowing ice-sheet on the Sierras is, I believe, proved by the domes and dome-like forms so abundant on the higher slopes of the Sierras, especially about Yosemite. These are the *roches moutonnées* of an ice-sheet which not only filled the cañons, but covered the peaks and domes—moulding itself upon their surfaces and determining their forms. While the great mass of this enormously thick sheet flowed in one general direction with a steady current, its lower portions, or portions in contact with the earth, doubtless conformed more or less to the greater valleys.

The actual forms of a glaciated surface are determined not only by the nature of the eroding agent, but also by the character of the material eroded. This latter factor is extremely variable. Sometimes it is the dip of strata or of cleavage, sometimes relative hardness in different parts, sometimes it is other kinds of structure, which have the controlling influence. The slopes of the Sierras, where the general erosion cannot be less than several thousand feet vertical, afford an excellent opportunity of observing how resulting forms have been determined by the rock structure.

According to my observations, there are two striking peculiarities in the structure of the granite about Yosemite: one is a *concentric structure*, on a huge scale; the other is a coarse *perpendicular cleavage*. The former is beautifully seen in the Royal Arches, which consists of several concentric semi-circular division-lines or joints, of nearly 2000 feet radius, seen on the perpendicular walls of Yosemite: this structure gives rise to the domes and dome-like forms so prevalent in this region. The other gives rise to the *perpendicular cliffs* and spire-like forms so common in the same region. It is idle to speculate on the origin of Yosemite and of the domes in its vicinity, without taking this peculiar structure into account. The origin of Yosemite we will discuss presently. The dome-like forms are, it seems to me, the combined result of ice erosion and the concentric structure. Water-erosion, it is true, if sufficiently great, would probably have brought out these forms, but ice-erosion would do so *more perfectly*, both because it is a more powerful

agent and because it tends, under all circumstances, to determine forms of this kind.

As the glacial period waned, the universal snow sheet retreated to the summits, and the slopes of the Sierras were now occupied, east and west, by great but *separate* glaciers, several of which I have attempted to describe. These in their turn retreated upward and took refuge in the snow fields, nestled amongst the peaks and amphitheaters of the highest summits. There, their still remaining but feeble existence has escaped observation, until revealed by the indefatigable industry and keen scrutiny of Mr. Muir.

During the fullness of the glacial period, I suppose glacial or *moutonnéed* forms covered every portion of the Sierras, even to the summits. These forms have not been retained on the summits, because they have been subsequently modified by continuance of snow in this region. The evidences of *universal* glaciation, therefore, must be looked for only on the higher slopes.

B. *Origin of Yosemite.* — Whitney thinks* Yosemite was formed by sudden *engulfment* of a portion of the slope of the Sierra: he believes that it is impossible otherwise to account for its nearly vertical walls. My own observations, on the contrary, lead me to believe that it has been produced by glacial erosion. My reasons for believing so are briefly as follows:

It is a remarkable fact that many of the great glacial valleys of the Sierras become deep narrow cañons with precipitous walls *near the junction of the granite with the slates* which lie on the *lower* slopes. This is the position of Yosemite. Similarly the Tuolumne glacier valley, near the junction of granite with the slate, becomes Hetch-hetchy, a valley very similar to Yosemite, and almost rivaling it in grandeur. Again, the valley of the American river becomes very deep, narrow, and precipitous just before reaching the slate. How general this is I do not know, but the fact that all these cañons, with nearly vertical walls, have been *occupied* by glaciers, makes it almost certain that they have all been *formed* by this agency. If Yosemite were unique, we might suppose that it was formed by violent cataclysm: but *Yosemite is not unique* in *form*, and therefore probably not in *origin*. There are many Yosemites. It is more philosophic to account for them by the *regular* operation of known causes. I must believe that all these deep perpendicular *slots* have been sawn out by the action of glaciers; the *peculiar verticality of the walls having been determined by the perpendicular cleavage structure* before spoken of.

* Geol. Survey, vol. i, p. 421. Yosemite Guide Book, p. 74.

C. *General structure of the Sierras.*

I have long believed that granite is but the last term of metamorphism of siliceous rocks,* and that metamorphism only occurs in deeply buried sediments, and therefore that where granite appears on the surface, as it so often does in great mountain chains, it has been exposed by immense erosion. It seems to me this is very clearly shown in the Sierras. There can be no doubt that the *slates* which now form a belt on lower slopes of this chain, *once covered the whole chain*, and have been removed by erosion. Along the very crest the slate still remains in patches, forming the highest peaks; such as Mt. Dana, Mt. Lyell, and many others. This crest, forming the divide of the snow-sheet, has been less eroded, and the slate therefore still remains, but on the higher slopes where the glacial erosion was universal and very great, the slate has been nearly all cut away and the granite left bare. A few patches of slate are left, however, even on these slopes to tell the story; such a patch is found, for example, on Mt. Hoffman. Only on the lower slopes, near the *limits of the ice-sheet*, does the slate appear again in force, as the auriferous slate belt. The limit of the granite region marks, I believe, the limit of the ice sheet.

The actual amount of general erosion it is perhaps impossible to know, but it has been evidently enormous. I think there is good reason to believe that in addition to the whole thickness of the slates, several thousand feet of underlying granite has been also removed. The highest peaks of the Sierras are composed of slates, but the valleys even in this region are composed of granite: the erosion here, therefore, has been through the slate into the granite. A little lower down, the Cathedral and other peaks and ridges in the vicinity are composed of very coarse and very feldspathic granite: the erosion here has been through this at least 2000 feet and down to the harder and more siliceous granite. Still lower down, about Yosemite, only this harder granite exists; both the slate and the softer granite has been entirely removed.

D. *Ice-erosion and water-erosion compared.*

The *great bulge* of the earth's crust which constitutes a mountain chain, is doubtless produced by general causes affecting the whole earth, probably by shrinkage of the interior more than the exterior, by which the face of the old earth becomes wrinkled: but the *smaller inequalities* are almost always produced entirely by *erosion*. How any one who has ever been amongst the high Sierras can for a moment doubt this fact, I cannot understand. Standing amongst these mountains, all

* On Great Features of Earth's Surface, this Jour., vol. iv, p. 467.

that constitutes scenery—every peak, ridge or dome; every valley or cañon—is evidently due to this cause alone, except in those parts where recent volcanic action has taken place.

Now the forms produced by erosion depend partly upon the *kind of rock*, and partly upon the *kind of erosion*. The forms determined by *water* are different from those determined by *ice*. Standing in the middle of the San Joaquin plains, on a clear day, the crests of the Sierras are seen on one side and of the Coast range on the other. Nothing can be more striking than the contrast between the two; the sharp saw-like, teeth-like outline of the former, and the rounded outlines of the latter. The reason is that the one has been determined by the action of snow and frost; the other by water only. The contrast is still more conspicuous when we are among these summits. In all the region where perpetual snow still lingers, the Sierras are studded with peaks, and spires and comb-like ridges, so sharp that they seem ready to fall over; and especially with great amphitheaters, bounded on several sides by almost perpendicular palisade-like walls, with sharp serrated edges.* These great amphitheaters are the wombs of ancient glaciers and are still filled with snow.

Such is the contrast in the summits. On the higher slopes the contrast is equally striking. During the period of the *ice sheet*, probably all the Sierra slopes, even to the summits themselves, were covered with rounded *moutonneé* forms on a grand scale. Subsequent action of ice and snow and frost and rivers have destroyed these, except in some localities. I am not now, however, speaking of the forms determined by the ice-flood, but of those determined since by *ice-streams* and *snow-fountains*. Water tends to form deep V-shaped cañons, while ice produces broad valleys with lakes and meadows. A camping party in the Sierras is made painfully aware of having passed beyond the limits of ancient glaciers by the sudden and entire disappearance of meadows and therefore of grazing for horses. Speaking in a very general way, water, it seems to me, tends to pro-

1. 2.

duce forms like fig. 1, while ice in the form of glaciers and eternal snow tends to the reverse forms of fig. 2. I know not how general these distinctions may be, but certainly the Coast range of this State is characterized by rounded summits and ridges, and deep

* These amphitheaters occur, I believe, only in the *slate* of the summits. The tendency to *perpendicular cleavage* is no doubt, therefore, a necessary coöperating cause of this form: but the sharp peaks and spires about Cathedral peak are all granite.

V-shaped cañons, while the high Sierras are characterized on the contrary by sharp, spire-like, comb-like summits, and broad valleys; and this difference I am convinced is due in part at least to the action of water on the one hand, and of ice on the other.

ART. XXXVII. — *Meteorological effects upon the heights of the Tides;* by WM FERREL. Letter to Professor BENJAMIN PEIRCE, Superintendent U. S. Coast Survey. (Communicated by the author with the authority of the Superintendent of the Coast Survey.)

CAMBRIDGE, MASS., Jan. 2, 1873.

Dear Sir: I have the honor to submit the following preliminary report of the principal results obtained from a discussion of a series of six years of the tidal observations in Boston harbor, with regard to the meteorological effects upon the heights of the tides.

The heights of high and low water of the tides for six years, from 1856 to 1861 inclusive, were computed by the formulæ and tables given in a previous report, and the results compared with the observations and the residuals noted. From the meteorological records of Cambridge Observatory for the same years, the corresponding barometric pressures and the directions and forces of the winds were obtained, and collated with the residuals. The range of the barometer was then divided into seven parts, and all the barometric pressures belonging to each one of these divisions were grouped together, and likewise the corresponding tidal residuals, and the averages taken and compared. High and low waters were at first kept separate, but the results subsequently combined. The observations also when the pressure was increasing, and those when it was decreasing, were kept separate in order to determine whether there is any perceptible difference on account of the inertia and the friction of the water preventing it from assuming at once the condition of static equilibrium. The following table of combined results of high and low waters were thus obtained.

Rising Barometer.			Falling Barometer.			Averages.	
No. of Obs.	Barometric pressures.	Tidal Residuals in feet	No. of Obs.	Barometric pressures.	Tidal Residuals in feet.	Barometric pressures.	Tidal Residuals in feet.
158	29·500	+0·307	279	29·480	+0·320	29·487	+0·315
363	29·708	0·152	569	29·717	0·212	29·713	0.192
449	29·842	+0·050	519	29·843	0·082	29·843	0·067
662	29·943	−0·030	695	29·940	+0·035	29·942	+0·002
887	30·053	0·062	820	30·050	−0·035	30·052	−0·049
962	30·197	0·115	845	30·190	0·125	30·195	0·120
628	30·420	−0·260	472	30·415	−0·240	30·418	0·251

In taking the averages in the last columns regard was had to the number of observations. The average barometric pressure is 30·007 inches. If, therefore, we put p for the pressure belonging to the average of any of the groups of observations, we shall have for the correction of the height of the tide $(30{\cdot}007-p)C$, in which C is a constant to be determined from observation. This expression put equal to each of the average residuals in the last column, reduced to inches, with the corresponding pressures or values of p, gives seven equations for determining the probable value of C. Giving the equations weight according to the number of observations, we thus get $C=7{\cdot}2$.

By comparing the residuals of rising barometer with those of falling barometer in the preceding table, it is seen that there is a perceptible difference near mean barometer, and that consequently the sea-level when the barometer stands at the mean and is rising is a little lower than when it stands at the mean and is falling. This difference, however, is very small, and indicates that the sea, under the average rate of the change of barometric pressure, very nearly assumes the condition of static equilibrium.

The theoretical value of the constant C above is 13·56, this being the ratio between the densities of water and mercury. I am unable to explain why its value in Boston harbor is little more than half this value. There can be no sensible amount of error in the constant as determined from so many observations. Any error in the tidal formulæ and tables with which the heights of the tides were computed and the residuals used in the discussion obtained, cannot affect sensibly the result, since all such small inaccuracies are eliminated by the great number of observations used along with other abnormal disturbances which are much larger. The difference also between the preceding value obtained from observations and the theoretical value, cannot be accounted for by the effects of inertia and friction, as is shown by a comparison of the results belonging to rising and falling barometers.

The value of this constant, as determined for several ports, is also much greater. M. Dausy found that at Brest the ocean rises 0·223 of a meter for a depression of 0·0158 of a meter in the mercury. (*Connaisance du Temps*, 1834.) This gives 14·1 for the value of this constant at Brest. Mr. Bunt, also, from a discussion of the residuals between theory and observations for three years at Bristol, obtained 13·4 for the value of this constant. (*Eleventh Report of the British Association*, p. 31.) Lubbock found 11·1 for Liverpool, but at London he found that the water rises 6·3 inches for a depression of 0·90 of an inch of the mercury. (*Phil. Trans.*, 1836, p. 11.) This gives only

seven for the value of the constant, which is nearly the same as the value obtained for Boston harbor.

In order to obtain the effect of the winds upon the heights of the tides, all the residuals for the six years belonging to each of the eight principal points of the compass were grouped together, and also the corresponding averages of the forces of the winds and of the barometric pressures. The forces of the wind in the observations were represented by the numbers 0, 1, 2, 3 and 4, 0 denoting a calm, and 4 the strongest winds recorded. The following results were thus obtained.

Wind.	No. of observations.	Average force of wind.	Barometric pressure.	Average residuals.	Corrected residuals.
N.	244	1·6	30·007 + ·005	+0·21	+0.21
N.E.	317	1·7	+ ·009	0·23	0.24
E.	274	1·3	+ ·055	0·04	0·07
S.E.	131	1·5	+ ·053	+0·02	+0·05
S.	165	1·8	− ·074	−0·03	−0·07
S.W.	796	1·5	− ·021	0·10	0·11
W.	677	1·6	− ·073	0·18	0·22
N.W.	527	1·6	+ ·001	0·11	−0·11
Calm.	346		+ ·051	0·01	+0·02

The number of observations in the second column denotes the relative frequency of the winds from the different points of the compass, and the third column gives the average force of each wind according to the scale which has been given. The fourth column gives the mean barometric pressure, and the pressure for each wind. It is seen that with winds from N.W. around to S.E. the barometer stands above the mean, and from S.E to N.W., below the mean. The tidal residuals it is seen follow very nearly the same law. The full effect of the winds, therefore, is not represented by the fifth column, since they are generally affected by the corresponding deviation of the barometer from the mean, and require a slight correction by the preceding formula with the constant as determined. The last column contains the corrected residuals, which show the effects of the different winds having the average force given in the third column.

It is pretty generally thought that the winds cause very considerable changes of the sea-level, but it is seen from the last column of the table above, that an average N.E. wind raises the sea-level only about three inches, and a S.W. wind depresses it not quite so much. If the numbers in the scale of forces represent the velocities of the winds, the elevating and depressing effect of the winds may not be proportional to the forces, so that a very strong wind, denoted by four in the scale, may raise or depress the sea-level three or four times as much as a wind of the average force, this depending upon the

law of friction between the wind and the water. Very strong winds, therefore, may change the sea-level in Boston harbor a foot or more, and this agrees well with individual observations. Of about 700 tidal residuals of high water throughout the year 1859, obtained from a comparison of computation by the formulæ and tables, with observations, only 10 amount to as much as one foot. If therefore, we, suppose that these residuals are due to the effects of the winds only, and no part of them to other disturbing causes, and to errors of the tidal formulæ and tables, even upon this supposition we know by actual measurements with the tide-guage, that in the course of a whole year, the sea-level of Boston harbor is not often changed by the winds as much as one foot. The popular impression of large changes of sea-level, no doubt arises from making observations on sloping beaches where a small perpendicular change of level produces a very great apparent change.

An important meteorological result is shown in the fourth column of the preceding table, which is that the barometer during calms stands very near the maximum of all the averages of the winds from the different quarters. This indicates that the winds generally are of a cyclonic character, prevailing mostly in the interior of the cyclone where there is barometric depression, and that the calms are mostly in the external part where there is high barometer.

The following table of results, brought out in the discussion, shows the annual changes of the barometer. As the unreduced observations were used, a correction for temperature in this case has to be applied, to reduce the barometer to the mean of the year to correspond with the preceding results in which the average corresponds to the mean temperature, or to reduce them, as usual, to the temperature of freezing. In the following table the reduction is to freezing and for capilarity.

Month.	No. of observations.	Barometer.	Red. to 32° and for capillarity.	Corrected barometer.
		in.		in.
January	324	29·995	−·004	29·934 + ·057
February	310	30·007	·020	+ ·053
March	363	29·886	·043	−·091
April	347	29·957	.066	−·043
May	354	30·013	·090	−.011
June	361	29·961	·110	−·083
July	354	30·020	·121	−·035
August	377	30·023	·119	−·030
September	360	30·104	·104	+ ·066
October	358	30·058	·076	+ ·048
November	335	29·947	·046	+ ·067
December	320	30·055	−·020	+ ·001

The mean barometer at the height of 71 feet, and reduced to the temperature of 32°, is 29·934 in. Adding 0·082 in. for the

reduction to mean sea-level, we get 30·016 in. for the mean height of the barometer at the mean sea-level in Boston harbor.

The last column in the table shows that there is a very small term with an annual argument and a coefficient of about 0·05 in., making the barometric pressure a minimum about May and a maximum about November. The number of observtions was not sufficient to eliminate the accidental irregularities sufficiently to determine this small annual inequality very accurately, but it is evidently too small to account for much of the observed annual inequality in the mean sea-level.

The following table of results is obtained from classifying the observations of the wind according to their directions, for each of the four seasons and for the whole year.

Season.	N.		N.E.		E.		S.E.		S.		S.W.		W.		N.W.	
	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.
Winter	92	153	51	75	22	37	23	36	36	68	189	288	221	378	193	337
Spring	68	117	120	183	106	137	45	63	57	109	183	293	181	323	113	210
Summer	34	52	85	142	101	143	37	61	50	67	269	416	150	238	104	176
Autumn	60	97	73	129	51	82	31	48	43	75	188	309	167	275	156	264
Whole year	254	419	329	536	280	399	329	208	86	319	829	1306	719	1214	566	987

The number of observations denotes, also, the relative frequency of the winds from the different points of the horizon. It is seen that the predominating winds are from W. and S.W. during all seasons of the year. The numbers headed S. F. are the sums of the numbers in the observations denoting the forces of the wind. There is some uncertainty with regard to the scale used by Professor Bond in denoting the forces, but it is supposed to be the scale from 0 to 6, in which 0 denotes calm, and 6 a velocity of 85 miles per hour, the numbers representing the forces being nearly proportional to the velocities. At any rate the sums of the forces above may be regarded as representing the relative sums of the distances passed over within the limits of the errors of such observations. With a table of latitude and departure, therefore, we readily determine the relative distances passed over and the directions, for each season of the year and for the whole year. We thus get the following table of directions from which the wind blows and the relative distances traveled.

Winter	N. 78° W.	726
Spring	N. 85 W.	386
Summer	S. 71 W.	383
Autumn	N. 84 W.	498
Whole year	N. 87 W.	1920

It is seen that during the winter the wind blows from a point 12° N. of W., but in the summer from a point 19° S. of W.

This difference is caused by the difference in the relative temperatures of the land and sea in the two seasons, and shows that the winds have slightly a monsoon character. During the spring and fall, when the relative temperatures are about the same, the winds blow very nearly from the same point, which corresponds very nearly with that of the resultant for the whole year, which is 3° N. of W. The atmosphere in the course of the whole year moves a little more south than north. Dividing 1920 by 3299, the whole number of observations, we get 0·58 for the average force in the direction of the resultant. This, by the supposed scale used corresponds to a velocity of about eight miles per hour, in a direction a little south of east.

Very respectfully yours,

WM. FERREL.

Professor Benjamin Peirce, Sup't U. S. Coast Survey.

ART. XXXVIII.—*On the Origin of Mountains;* by JAMES D. DANA.

THE remark, that in Professor Hall's theory of the origin of mountains the elevation of mountains is left out (cited disapprovingly by Prof. Hunt on page 266), was made by me in volume xlii, of the last series of this Journal (p. 210, 1866), and not without due consideration. The importance which some still attribute to this theory makes it desirable that its special points should be stated here with more fulness than they were in the notice referred to, and that they receive further consideration. The points in the theory, selected out and condensed in the statement from the Introduction to the third volume of Professor Hall's New York Paleontology (where alone they have been published), are the following:

1. The Paleozoic strata of the Appalachian region bear evidence that they were mostly of shallow water origin.

2. Their great thickness, consequently, was attained through a slowly progressing subsidence, the axis of which was in the direction of the Appalachian chain.

3. This slowly progressing subsidence was occasioned by the weight of the slowly and successively accumulated sediments. The memoir says (p. 69):

"When these ["accumulations of sedimentary matter"] are spread along a sea bottom, as originally in the line of the Appalachian chain, the first effect of this augmentation of matter would be to produce a yielding of the earth's crust beneath, and a gradual subsidence would be the consequence."

4. This subsidence produced, as one of its direct results, the extensive folds and faults of the strata characterizing the Appa-

lachian formations. The fact that the folds are generally steepest on the northwest side may also be thus accounted for.

5. The formation of the Appalachians (and so of all mountains) was dependent upon, and the height related to, the thickness of the sedimentary accumulations, of which they are made; and a mountain chain was not a possibility over the Mississippi basin, because there "the materials of accumulation were insufficient."

6. (*a*) The elevation of the Appalachian mountains was not a result of the process of accumulation, or of the subsidence. (*b*) The elevation of mountains is, "of continental, and not of local, origin; there is no more evidence of local elevation along the Appalachian chain than there is along the plateau in the west." Again, the chapter says (p. 96): "It is this ultimate rising of continental masses that I contend for, in opposition to special elevatory movement along the lines of mountain chains." (*c*) After a continental elevation, the mountain range received its present shape mainly through erosion.

7. Metamorphism requires first large accumulations of rock material; and it went forward in the Appalachian region in consequence of the subsidence. As to the causes, Professor Hall says (p. 77)—after alluding to the view of Babbage and Herschel respecting a regular increase of temperature below following an increase in thickness of surface accumulations—"Such an increase of temperature would be much less than that usually supposed necessary for producing metamorphism; and it is extremely doubtful if any portion now exposed to observation ever reached a temperature much above that of boiling water. We must, therefore, look to some other agency than heat* for the production of the phenomena [of metamorphism] witnessed; and it seems that the prime cause must have existed within the material itself; and that the entire change is due to motion, or fermentation and pressure, aided by a moderate increase of temperature, producing chemical change."

A. The last of these propositions is an expression of the opinion that, in some way not understood, the heat required for metamorphism was generated within the strata that were altered. The effect was restricted by Vose in his Orographic Geology to pressure; but, with each of these authors, this pressure was due to the progressing subsidence referred to in the third proposition.

B. The first two propositions have general acceptation among American geologists.

C. The third and fourth, which are fundamental in the theory, have been shown in my former paper to be, as I

* On page 87, Hall observes that the lower beds may be *softened* by the heat that is received from below in consequence of accumulation above; but this remark is introduced not to make the heat a source of metamorphism, but to give a reason for the rocks yielding and subsiding under accumulating deposits, notwithstanding the property of heat ordinarily to cause expansion.

believe, physical impossibilities; and LeConte, in his recent article, has further enforced this opinion. The third assumes that the first 500 feet in depth of sediments would press down the crust 500 feet, and so on to the end; but no reason is given why sediments under water should have so immense gravitating power, when the crystalline rocks of the Adirondacks, piled to a height of some thousands of feet *above* the water, had a firm footing close along side of the subsiding region.

But while the weight of accumulating sediments will not cause subsidence, a slow subsidence of a continental region has often been the occasion for thick accumulations of sediments.

D. The fifth proposition announces a relation between the height of the mountains formed over an area, and the thickness of the sedimentary accumulations there previously made, preparatory to the elevation; and it further makes the absence of mountains from a region a consequence of small accumulations.

The relation set forth is a true and important one if taken in the most general way; but the application of it as a strict ratio, or as a universal law, encounters many apparent exceptions. In the Green Mountains of Vermont, the latest rocks of which are Lower Silurian, the conformable Silurian beds constituting them are probably not over half the thickness of those of the Appalachian region in Pennsylvania and Virginia; and yet the average height is greater, although exposed to erosion for a vastly longer time. For most mountain regions we have not the facts needed for the comparison—the thickness of the sediments preceding their formation and the amount of erosion since undergone being alike unknown. In some cases of composite mountain masses, like that of the Rocky Mountains, the principle hardly admits of application at all. According to King and others there were 10,000 feet in thickness of Cretaceous deposits laid down in Utah, with coal beds in the upper part of the series proving that subsidence accompanied the deposition; but, while there is evidence of subsequent disturbance in the region, and of the elevation since of a large part of the Rocky Mountain chain, there is, I believe, none that the Utah Cretaceous was raised into mountains overtopping the older ridges of the summit. There are, besides, cases of low plains, like the part of the Patagonian Pampas bordering the Atlantic, where for all that science knows, or ever will know, there may be a great thickness of conformable beds beneath. Again, there are areas now rising, like the coast region of Sweden, where thick accumulations of Tertiary sediments, or of any others since the Paleozoic, are wanting. There are many cases where the highest summits of a region occur at the crossing of two lines of elevation, showing that force has confounded the ratio of height to thickness of sediments, if any such had existed there.

Still, it is evidently a common fact that where mountains have been raised, there, in general, thick accumulations of sediments were previously made; and conversely. But the principle properly stated is—Where mountain ranges have been raised, there, in general, great subsidences, giving opportunity for thick accumulations of sediments, previously occurred; and conversely.

Unless the third and sixth of the above propositions are true, mountains are absent from the Mississippi because no local elevation extensive enough took place there; and no elevation was made, and there were likewise only thin accumulations of sediments, because the region, owing to its interior position, was not within the range of the continental-border oscillations.

E. By the sixth proposition, the influence of erosion in shaping a mountain is recognized, but no provision is made for its elevation—this elevation apart from general a continental elevation being denied. Thus Professor Hall's theory is strictly a "theory of the origin of mountains with the elevation of mountains left out." It accounts for plications by simple subsidence, but supposes the continent to get up high some way—the way not considered—without other plications, or any local uplifts, and on a crust so flexible that it will sink a foot for every foot of sediment added to the surface. The world abounds in cases in which part of the sea-border deposits of a period are now but little away from the old level, while other portions are many thousands of feet above it: *e. g.*, the Cretaceous strata of the United States; and it contains examples of modern rising of land. No principle has been found to explain such facts except that of *local elevation*.

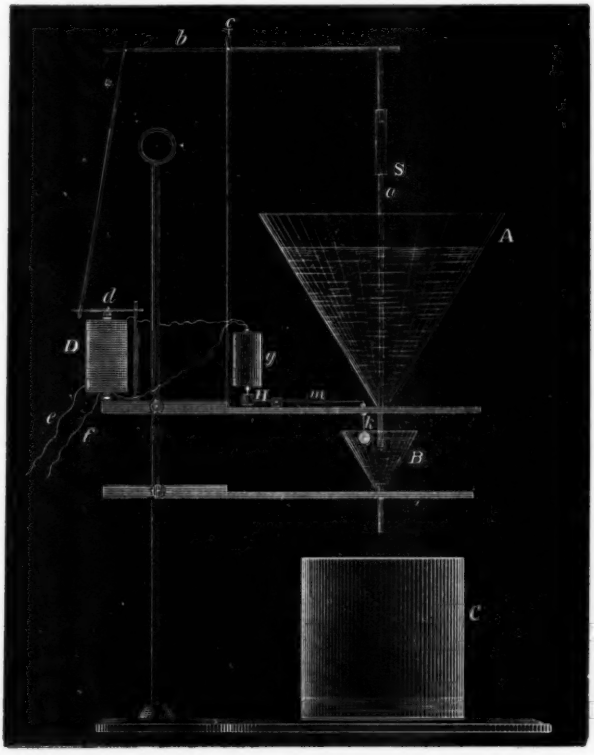
Since, then, the exposition which Professor Hall has made of his views offers nothing in explanation of the elevation of mountains—the event upon which the existence of any mountain depends—and since the only agent of change of level appealed to is one producing subsidence, and this will not work, so that there is no chance from it for the thick accumulations needed, or for the faintest plications or metamorphism, we may with reason pronounce the theory seriously deficient and defective.

ART. XXXIX.—*On an Automatic Filtering Apparatus;* by HARVEY W. WILEY, M.D., Professor of Chemistry, Indiana Medical College.

WHILE studying quantitative analysis in the laboratory of the Lawrence Scientific School, I was led to devise some plan by which time could be economized in filtering and washing precipitates.

Neither Mariotte's bottle nor the Bunsen pump were capable of general application.

As a result of a series of experiments the following apparatus was devised and successfully put into operation.



An ordinary filter-stand, with two arms, is fitted with a large glass funnel, A, of from one to four liters capacity. The lower arm holds a common small Bunsen funnel, B, so placed that the axes of the two funnels coincide. D is a small electro-magnet attached to the upper arm of the stand. The neck of the large funnel should be drawn out until it has an internal diameter of eight or nine millimeters. Passing down the center of the large funnel, and fitting into its neck is a glass rod, *a*, covered by a piece of a soft white caoutchouc connector. The glass rod is about twenty centimeters in length, and of such a size that when covered by the rubber hose it will completely close the orifice in the neck of the funnel.

This rod is connected by rubber with the lever, *b*, which is worked by the armature, *d*; *e* and *f* are two insulated copper wires connected with a small Grove, Bunsen or Daniel's cell; *e* passes through the magnet, *f* comes directly from the cell, and both at *g* are fastened into a small glass tube, the free ends denuded of their covering projecting three or four millimeters below.

The small funnel contains a glass bulb ten to fifteen millimeters in diameter, into which is soldered a small platinum wire. This wire is fastened to the long arm of the lever, *m*, the short arm of which carries a cup, H, holding a globule of mercury. The weight of the bulb, the amount of mercury, and the arms of the lever must be so adjusted, that when the small funnel contains no liquid the bob, *k*, will descend and the cup, H, rise, but when the small funnel is filling the relative weights of the two ends of the lever must allow the bob to rise with the liquid in the funnel B. This adjustment is easily effected by varying the amount of mercury in H. When the parts have all been properly connected, the glass rod, *a*, should exceed the weight of the armature, *d*, by such an amount as will enable it to thoroughly close, by its excess of weight, the neck of the funnel A. By means of the thumb screw, *c*, the lever, *b*, should be moved until the space between the armature and the poles of the magnet measures two or three millimeters. The small funnel, B, properly fitted with a filter paper, is fixed in the lower arm, which is raised and fastened in its place. The proper position for this lower arm is readily found by trial.

Let B be filled with distilled water as represented in the diagram. When the bob, *k*, stands as in the figure the mercury in H should not touch the copper wires. As the liquid runs out of B, the bob sinks, the cup rises until the wires touch the surface of the mercury. This immediately completes the electric circuit, the armature, *d*, is pulled down, the glass rod, *a*, elevated, and the liquid in A flows into B until the bob is raised to its first position. This breaks the circuit, the rod, *a*, falls, the flow of the liquid from A to B is stopped. After using distilled water for two or three minutes until everything is found properly adjusted, the substance to be filtered is poured into A, and no further attention is required until the whole of it has run through into B; *a* is then to be disjointed at *s*, and with the wash bottle, using *a* as a rubber, any precipitate adhering to A is easily washed through into B; B is then to be lowered and removed and washed.

Advantages of the Apparatus.

Washing by decantation.—The whole of the supernatant liquid can be poured at once into A. The beaker is refilled with the

washing fluid, and by the time the first portion has run through you are again ready to decant. By other processes filtration has to be suspended each time until the supernatant liquid is clear.

Washing precipitate on filter.—By means of the thumb screw, *c*, the apparatus can be adjusted to discharge jets of water from A with force enough to thoroughly stir up the precipitate each time, thus greatly facilitating the process.

Saving time.—This is the chief feature of the automatic filter. I give below some tabulated data taken from a great number of observed filtrations. In all these cases the results were as good as those usually obtained by hand and in several instances, owing to special care, better.

I. Washing precipitate $BaSO_4$ by decantation.

Adjustment 5 min., filling 3 min., washing 12 min., total 20 min. Time apparatus at work, 3 h. 15 min. Amount wash water, 2 liters 600 $\overline{\text{c.m.}}^3$.

II. Washing precipitate $Fe_2O_6H_6$.

Adjustment apparatus 2 min., filling 3 min., washing 15 min., total 20 min. Time, apparatus at work, 2 h. 40 min. Amount filtrate 2 lit. 200 $\overline{\text{c.m.}}^3$.

III. Washing precipitate $BaCl_2$.

Adjustment 4 min., filling 2 min., washing 18 min., total 24 min. Time apparatus at work, 1 h. 40 min. Amount filtrate 1 liter 250 $\overline{\text{c.m.}}^3$.

IV. Washing $Al_2O_6H_6$.

Adjustment 1 min., filling 1 min., washing 5 min., total 7 min. Time apparatus at work, 1 h. 50 min. Amount wash water 525 $\overline{\text{c.m.}}^3$.

While at work the apparatus requires no attention whatever. It can be adjusted so that the height of the liquid in B will not vary more than a millimeter. By fitting B with a hot water jacket, which can be very easily done, precipitates which easily oxidize can be thoroughly washed and kept at 100° out of contact with the air.

The whole cost of the apparatus need not exceed three or four dollars, and usually all that will have to be bought is the electro-magnet. Other necessary parts will be found in the laboratory.

I am indebted to the kindness of Mr. Ed. H. Ketcham of Dartmouth College for the accompanying drawing.

Art. XL. — *Investigations on Parasulphobenzoic Acid;* by Ira Remsen.

(Concluded from page 282.)

V. *Attempts to prepare Orthosulphobenzoic Acid.*

The fact that in the crude sulphotoluenic acid, employed in the preparation of parasulphobenzoic acid as above described, two varieties are contained, viz: the ortho- and para-; and, further, the fact that the crude product of the oxidation, in the form of the potassium salts, when fused with potassium hydroxide yielded, as we have seen, salicylic as well as paraoxybenzoic acid, showed plainly that the ortho-acid had not been destroyed by the oxidizing agent. The process was in other cases continued for a long time, and a large excess of the oxidizing mixture employed; and still salicylic acid was obtained, its quantity as compared with that of paraoxybenzoic acid appearing rather to be increased than diminished under these circumstances.

This result was not what might have been anticipated after Fittig had called attention to the fact that ortho-compounds conduct themselves toward oxidizing agents differently from the compounds of the two other series, the former being, as he states, completely destroyed, yielding carbonic acid and water. In the case under consideration two explanations might be given; either the orthosulphotoluenic acid had been converted into orthosulphobenzoic acid; or it had withstood the action of the oxidizing mixture; for orthosulphotoluenic acid itself when fused with potassium hydroxide yields salicylic acid, if the heating be continued long enough, as was shown by Wolkow (loc. cit.).

The solution which yielded the acid barium parasulphobenzoate was evaporated down gradually, and, after it was reduced to a small volume, it was still found to contain a considerable quantity of an easily soluble salt, differing entirely from the difficultly soluble parasulphobenzoate. This was recrystallized a number of times, and finally obtained in the form of microscopic needles; though even after repeated recrystallizations its appearance hardly warranted the conclusion that it was a pure compound. It was, as stated, easily soluble in water; and the difference between its solubility in cold water and that in hot was not very great; so that the crystallization was necessarily brought about by allowing the solution to stand for a length of time over sulphuric acid. The salt thus prepared was analyzed after being thoroughly dried over sulphuric acid.

0·4215 grams of the salt, on being heated to 240°, lost 0·0155 grams H^2O; and then gave 0·2228 grams $BaSO^4$=0·121 Ba.

	Calculated.		Found.
$(C^7H^7SO^3)^2$	342	68·80	
Ba	137	27·58	28·71
H^2O	18	3·62	3·68
	497	100·00	

Though the results of this analysis agree but poorly with the calculated percentages, it nevertheless makes it appear exceedingly probable that this easily soluble substance is nothing but the salt of orthosulphotoluenic acid. Acid barium sulphobenzoate requires much less barium (23·10 per cent) and much more water (3 mol.=9·11 per cent). The fact that more barium was found than is required by the theory would appear to indicate that the salt was rendered impure by the presence of a small amount of a neutral barium salt of sulphobenzoic acid. As the neutral barium salt of parasulphobenzoic acid corresponds in its properties very nearly to the salt here described, this becomes still more probable. Be this as it may, it is evident that the CH^3 group of orthosulphotoluenic acid has not been acted upon by the oxidizing mixture, nor has the acid been destroyed. The same results were obtained when a large excess of sulphuric acid and potassium bichromate were allowed to act upon the mixture of the two sulphotoluenic acids for a long time; so that the statement of Fittig that the ortho-compounds are destroyed by oxidizing agents requires qualification. It seems indeed from this and a subsequent experiment, that the ortho-acid is acted upon with much less energy than the para-acid, if acted upon at all. This case agrees, however, with those mentioned by Fittig in the fact that the toluene-derivative yields no corresponding derivative of benzoic acid.

The destruction of aromatic compounds is by no means confined to those which belong to the ortho-series. The case of salicylic acid mentioned by Fittig is indeed no proof of his general statement, inasmuch as all aromatic oxyacids, as far as I have subjected them to experiment, are destroyed with equal facility. I have, for instance, treated salicylic, oxybenzoic, paraoxybenzoic, protocatechuic and gallic acids with sulphuric acid and potassium bichromate, and in each case exactly the same phenomena were observed. Gentle heating was sufficient to commence the process, which then proceeded violently to the end, accompanied by an evolution of carbonic acid, without the further aid of heat. On now examining the mixture, not a trace of a solid product could in any way be discovered. So that salicylic acid does not differ in this respect from other aromatic acids containing OH; and its conduct toward oxidizing

agents is of course no proof that it belongs to the ortho-series. This does not, however, detract from the value of Fittig's exceedingly interesting observation; it merely serves to define it more accurately.

That the ortho-compound in the special case under consideration is acted upon less energetically than the para-compound, was also shown in a rough way by the following experiment. The potassium salts of the two sulphotoluenic acids were separated very nearly by means of crystallization. The pure para-salt was introduced into the oxidizing mixture, and the reaction that ensued carefully observed. The same quantity of ortho-salt, still containing some of the para-salt, was afterward introduced into the same quantity of the oxidizing mixture as was employed in the former case, and the reaction compared with the former one. A striking difference was observed. Whereas the reaction commenced very quickly with the para-salt, and a strong evolution of gas took place; with the ortho-salt it was necessary to apply heat for a longer time before the reaction fairly began, and then the process was markedly more sluggish, a very slow evolution of gas continuing for a long time. By means of approximate quantitative experiments, further, it was shown that the longer heat was applied to the vessel in which the oxidizing process was going on, the smaller was the yield of para-acid; but in no case did I succeed in completely destroying either the ortho- or para-acid. This shows that the para-acid is very susceptible to the action of the oxidizing agent, the oxidation taking place apparently at first in the methyl-group and then extending gradually to the whole molecule; whereas the ortho-acid resists the same influence strongly, and if oxidation takes place at all, it breaks up the compound, yielding the last products of combustion. Whether the same is true of other cases I am unable to say; facts do not speak against it at present, and further experiment can alone decide this point, which certainly possesses more than ordinary interest.

I was thus forced to abandon the hope of obtaining orthosulphobenzoic acid by the oxidation of orthosulphotoluenic acid by means of sulphuric acid and potassium bichromate. It is possible that other oxidizing agents, as for instance potassium hypermanganate may yield more satisfactory results. A preliminary experiment made with this salt in an alkaline solution showed that oxidation took place readily; and I shall soon commence the study of this reaction in detail.

VI. *Oxidation of the Amides of Sulphotoluenic Acid.*

The difficulty with which ortho-compounds are obtained and the importance of studying them in a pure condition, in order to complete our knowledge of their conduct under various con-

ditions, led me to undertake one more experiment, with the hope of finding a method for the preparation of orthosulphobenzoic acid. The experiment failed to bring about the desired object, but at the same time gave other interesting results, an account of which follows.

According to A. Wolkow (loc. cit.) the amides of the two sulphotoluenic acids can be separated from each other by means of crystallization. This statement offered a prospect of obtaining an ortho-compound in pure condition; and I hence prepared a quantity of the amides from the crude mixture of potassium salts of sulphotoluenic acids. The perfect separation of the two by means of crystallization is an exceedingly tedious operation, whether water or alcohol be employed as the solvent. From water the paramide crystallizes first, and can then easily be purified; from the mother-liquor a mixture of the two amides is deposited. This fuses at 124°, and can only be resolved into its constituents by a very long series of recrystallizations. I succeeded at last in obtaining a small quantity of the ortho-amide in a pure condition, with all the properties as given by Wolkow.

Now as the amide contains the group $SO^2.NH^2$ instead of the sulpho-acid group $SO^2.OH$, it seemed possible that its conduct toward oxidizing agents might differ from that of the sulpho-acids. No attempts had as yet been made to oxidize compounds containing such a complicated group as $SO^2.NH^2$, and I was obliged to gain a certain amount of preliminary knowledge before proceeding to subject the ortho-amide to oxidation. For this purpose I introduced a few grams of parasulphotoluen-amide into a noted amount of the oxidizing mixture (sulphuric acid and potassium bichromate); and heated the whole gently for a short time. Soon the oxidation began, as was shown by a change in color and an evolution of gas. The amide dissolved rapidly, and, soon after it had completely disappeared, a beautifully crystallized product began to make its appearance, and increased constantly in quantity. After cooling, the liquid was filtered off. The solid product remained on the filter, after being washed out with cold water, in a pure white condition. It consisted of beautiful, short, lustrous prisms that bore no resemblance to the original amide. It was found to be easily soluble in alkaline carbonates, carbonic anhydride being evolved, and was reprecipitated from these solutions in crystalline form on the addition of mineral acids. It was almost insoluble in cold water and difficultly soluble in hot water; and when only once crystallized from water, it had the form of flattened prisms, sometimes more than an inch in length. These had a high luster, and while in the solution exhibited a very beautiful fluorescence. It was found to fuse at a very

high temperature, but to undergo decomposition before this point was reached. These properties distinguish the new substance from the amide of parasulphotoluenic acid. Its composition was determined by the analysis.

I. 0·8657 grams of the substance, thoroughly dried over sulphuric acid, were oxidized in a silver basin with saltpetre and potassium hydroxide (Liebig's method). There were obtained 1·03 grams $BaSO^4$=0·14146 grams S.

II. 0·5505 grams of the dried substance were heated with soda lime, and the vapors collected in dilute hydrochloric acid. In this way were obtained 0·262 grams Pt, corresponding to 0·037163 N.

	Calculated.		Found.
$C^7H^7O^4$	155	77·11	
S	32	15·92	16·34
N	14	6·97	6·75
	201	100·00	

These results agree with the formula $C^6H^4\begin{cases}SO^2.NH^2\\CO.OH\end{cases}$, and this formula agrees with the method of formation of the substance. We have:

$C^6H^4\begin{cases}SO^2.OH\\CH^3,\end{cases}$ $C^6H^4\begin{cases}SO^2.NH^2\\CH^3,\end{cases}$ and $C^6H^4\begin{cases}SO^2NH^2\\CO.OH.\end{cases}$

Sulphotoluenic Acid. Sulphotoluenamide. New Acid.

I have given the acid the name *parasulphaminbenzoic acid*, as indicating the constitution which it undoubtedly possesses.

A compound of a similar constitution, but belonging to another series, is already known, viz: the so-called *sulphobenzamic acid*. This was prepared by Limpricht and Uslar* and by Engelhardt.† The former obtained it by heating sulphobenzamide, $C^6H^4\begin{cases}SO^2.NH^2\\CO.NH^2,\end{cases}$ or ammonium ethylsulphobenzoate with potassium hydroxide; further, by treating the compound $C^7H^6N^2SO^2$ (obtained from sulphobenzamide by treating with phosphorus chloride) with caustic potassa. In both cases ammonia is eliminated. Engelhardt obtained it by means of a complicated reaction, viz: the action of sulphuric anhydride on benzonitrile, other products being formed at the same time. Sulphobenzamic acid being derived thus from ordinary (meta) sulphobenzoic acid retains in all probability the meta-position of the substituting groups; and, although it is not proved by experiment, it is probable that the amide-group is in this case also situated in the sulpho-group and not in the car-

* Annalen der Chemie u. Pharmacie, cvi, 27.
† Jahresberichte, 1858, S. 278.

boxyl: the name would indicate the contrary, but the analogy that this compound shows with parasulphaminbenzoic acid, in which the amide group is undoubtedly situated in the sulpho-group, leads me to believe that the name is incorrect; and that the name metasulphaminbenzoic acid would be more in accordance with the internal character.

To the account given above of the method of preparation and the properties of parasulphaminbenzoic acid, I need only add a few details. For the oxidation the following proportions were found to be most favorable: To 20 grams potassium bichromate are taken 30 grams ordinary sulphuric acid, diluted with three times its volume of water; this mixture sufficed for the oxidation of 7 grams of the pure amide. The amide is introduced into the mixture after the latter has become cool. At first the flask is heated by means of a very small gas-flame. A moderately violent action takes place, and in a short time the amide is entirely dissolved; now in a few minutes the separation of the oxidation-product begins. The mass increases constantly in quantity until the liquid has the form of a thick paste. In about an hour from the beginning of the process the oxidation is completed. The whole is now allowed to cool down to the ordinary temperature, the product is filtered off and washed out with cold water, until the wash-water passes through colorless. On the filter is the parasulphaminbenzoic acid in the form of beautiful crystals, which only require to be recrystallized from water once in order to be made perfectly pure. It is easily soluble in alcohol and crystallizes from this solvent in smaller crystals than are obtained from water; and these do not exhibit the property of fluorescence. It is precipitated in crystals both from a hot and cold alcoholic solution by water.

Sulphobenzamic acid crystallizes, according to Limpricht and Uslar, in scales like potassium chlorate; according to Engelhardt in rhombohedral crystals or in needles, consisting of aggregates of small rhombohedrons. Heated above the melting point it volatilizes in white vapors. Distilled with phosphorus pentachloride it yields a number of products among which is metachlorbenzoyl chloride. It may be expected that parasulphaminbenzoic acid will under the same treatment yield parachlorbenzoyl chloride—a point which I shall decide by experiment.

Ethyl parasulphaminbenzoate, $C^6H^4 \begin{cases} SO^2.NH^2 \\ CO.OC^2H^5. \end{cases}$ This beautiful compound is the most characteristic derivative of the acid. It is prepared in the usual manner by conducting dried hydrochloric acid gas into a solution of the acid in absolute alcohol, and afterward heating gently on a water bath. Water

gives no precipitate in the solution thus obtained. The alcoholic solution must be evaporated down to the consistence of syrup; it then congeals on cooling, and consists of a mass of fine, colorless needles. In water it is less soluble than in alcohol; in cold water much less than in hot. When boiled with water it melts in the liquid before dissolving. From the aqueous solution it separates in the form of long, beautiful needles of a silken luster. These arrange themselves nearly parallel, and may attain the length of several inches. In connection with the melting point, this substance exhibits interesting though perplexing phenomena. In order to be sure that it was absolutely pure I subjected it to recrystallization a number of times; though each time it was obtained in the same form, and possessed in the highest degree the appearance of a chemically pure substance. On endeavoring to determine the melting point, however, I was surprised to find that this varied according to circumstances. When first heated it melted entirely at 110–111°; if it were now removed from the bath and allowed to solidify, it melted immediately after at 94–95°; the longer it was allowed to stand after the first melting, the higher the melting point became, until finally, in about two hours, it again reached 110–111°. Specimens examined at different intervals showed melting points which varied between the limits mentioned; every time that the substance was melted once and then allowed to congeal, and again immediately examined, the melting point was found to be 94·95°. This, taken together with the fact that the substance was pure, is certainly very remarkable. It is possible that this case belongs in the same category with that observed by Zincke* in connection with the two modifications of benzophenone, which is in its turn decidedly inexplicable.

Of the ether a sulphur estimation was made as follows:

0·2849 grams of the substance, dried over sulphuric acid, were oxidized with KOH and NO^3K (Liebig's method), and gave 0·2903 grams. $BaSO^4$=0·03987 S.

	Calculated.		Found.
$C^9H^{11}O^4N$	197	86·03	
S	32	13·97	13·99
	229	100.00	

Ethyl sulphobenzamate also crystallizes according to the description in "splendid, shining needles;" these were determined to be monoclinic prisms. No determination of the melting point appears to have been made. "It dissolves easily in warm alcohol; somewhat less easily in boiling water."

* Berliner Berichte, IV Jahrgang, 576.

Barium parasulphaminbenzoate, $(C^7H^5SO^4)^2Ba+H^2O$. This salt is prepared by boiling the acid with barium carbonate. It is easily soluble in cold and hot water, and crystallizes in nodular aggregations.

The analysis gave the following results:

0·497 grams salt, dried over sulphuric acid, lost, at 200°, 0·0161 grams H^2O; and gave 0·2094 grams $BaSO^4$=0·12313 grams Ba.

	Calculated.		Found.
$(C^7H^6SO^4N)$	400	72·08	
Ba	137	24·68	24·77
H^2O	18	3·24	3·24
	555	100·00	

The corresponding salt of sulphobenzamic acid crystallizes with $4H^2O$ as "a soft, wavellitic, crystalline mass;" it gives off its water at 110°.

Ammonium parasulphaminbenzoate, $C^6H^4\begin{cases}SO^2NH^2\\CO.ONH^4\end{cases}$, prepared by dissolving the acid in ammonia, is easily soluble in cold and hot water. It easily forms supersaturated solutions, which congeal on being disturbed. It crystallizes in beautiful needles or long laminæ.

The ammonium salt of sulphobenzamic acid crystallizes in laminæ.

The new acid is thus sufficiently well characterized as a chemical individual by these experiments; but as it represents a class of derivatives differing from others in their more complicated nature, it deserves a more thorough examination. It is susceptible to the action of ordinary reagents, and yields compounds with them. It can be boiled with concentrated nitric acid without undergoing change, it being thrown down in crystalline form on the addition of water. It is insoluble in fuming nitric acid at the ordinary temperature; but dissolves when heat is applied. On the addition of water to the solution no precipitate is formed; on evaporating, however, a very easily soluble, colorless, crystalline product was obtained. Fuming sulphuric acid also dissolves it with the aid of heat, and as water gives no precipitate with the solution, it is probable that a sulpho-acid is formed; though in this connection it should be remembered that the isomeric body, sulphobenzamic acid, appears to yield sulphobenzoic acid when treated with sulphuric anhydride (Limpricht and Uslar). The study of these reactions I shall take up in due time. Parasulphaminbenzoic acid having a symmetrical structure, the investigation of its sulpho-acid offers a possibility of throwing light upon the constitution of the tribasic acids of benzene; the conversion of the two sulpho.

groups into carboxyl may succeed, though, after the experience of Ascher* with sulphoterephtalic acid, this is hardly probable.

I have already stated that the object of undertaking the oxidation of the amides of sulphotoluenic acids was to open another road with the hope of arriving in the end at a method for the preparation of orthosulphobenzoic acid; I also stated that this object was not attained. Orthosulphotoluenamide still containing some of the paramide, was subjected to oxidation. It was immediately noticed that, as in connection with the sulpho-acids, the action in this case was not as violent as in the case of the para-compound. After heating for some time the product was examined. The parasulphaminbenzoic acid formed was filtered off and the filtrate extracted with ether. The ethereal extract, on being distilled, left behind a residue consisting of orthosulphotoluenamide with a very little parasulphaminbenzoic acid. The latter can be readily removed by redissolving the whole in water, adding a little alkali to the solution, and then again extracting with ether. In this way absolutely pure ortho-amide can be obtained. This was again subjected to oxidation, and, after treating for a length of time, no new product could be discovered in the liquid—a portion of the substance only having been destroyed. Thus we see that the conduct of the ortho-amide is exactly analogous to that of the corresponding sulpho-acid; and this serves to strengthen the conclusion drawn in regard to the conduct of ortho-compounds in general toward oxidizing agents.

The points which have been left unsettled thus far in this investigation will be considered in a second paper on a basis of experiments.

Williams College, Mass., December, 1872.

ART. XLI.—*The Salt deposits of Western Ontario;* by JOHN GIBSON, B.A., Principal of the Almonte High School, Ont.

Extent of the Salt-bearing area.—The superficial area of the Ontario salt deposits is comparatively small. Its northern, northeastern, eastern and southeastern limits seem to be pretty clearly definable by means of the numerous artificial perforations that have of recent years been made in this portion of the Province. From such observations it appears that the whole salt-bearing district may be included within the counties of Huron and Bruce. These portions of Ontario lie along the eastern shore of Lake Huron; and are bounded on the north and east respectively by the counties of Grey and Wellington,

* Annalen der Chemie u. Pharmacie, clxi, 3.

and on the south by the county of Lambton. Numerous borings in search of salt have, however, been made in other districts of the Province, but all such attempts have finally proved fruitless. Direct experiment, therefore, forces us to the conclusion that by far the greater volume of salt is to be found under the waters of Lake Huron, and this view is partly corroborated by the fact that at Port Austin, in Michigan, which lies almost due west of the Ontario salt region, there was extracted at the depth of 1,198 feet from the surface a brine marking, according to Dr. Gœssman, 88° salometer, and containing 17·61 per cent of sodium chloride. This latter perforation indicates at the outset sandstones of the Chemung period; but as the depth increases, certainty regarding the exact geological formation attained diminishes in like ratio. However, it may with some degree of plausibility be conjectured that in this boring, as in those in Ontario, the source of the brine is to be found in the Salina formation of the Upper Silurian series of rocks. Yet it would be altogether unsafe to state unhesitatingly that such really is the case, seeing that a very great diminution in the average volume of the strata overlying the base of the Salina formation is unmistakably presented. From the foregoing data it is extremely probable that this ancient geographical depression or salt-basin had an eastern and western extension of at least 85 miles, with probably a much greater stretch from north to south.

Geological features of this Salt area.—The fundamental rocks of this district belong, with but one or two exceptions, to the *Corniferous limestone formation* of the Middle Devonian system. These Devonian rocks of Ontario are represented by portions of the Oriskany sandstone, Corniferous limestone (including the Onondaga limestone), Hamilton, Portage, and Chemung groups. The following is given as a table showing approximately the geological position of the different formations observed, either as outcrops or by borings in the area in question; and it is given in full in order that it may include all the formations that present themselves in the numerous borings for salt in the vicinity, and that the relative position of any subdivision may at once be recognized:—

I. Middle Devonian,	Corniferous limestone, including the Onondaga limestone.
	Schoharie grit (not observed in Ontario).
II. Lower Devonian,	Cauda-galli grit (not observed in Ontario).
	Oriskany sandstone.
III. Upper Silurian,	Lower Helderberg group of Vanuxem, including only the fundamental *Tentaculite limestone.*
	Onondaga formation, Salina group of Prof. Dana.

IV. Middle Silurian,	Guelph and Galt formations.	Anticosti group.
	Niagara formation.	
	Clinton "	
	Medina "	

Of the subdivisions of the Middle Devonian system one is only found within the area under review. This is the *Corniferous limestone* formation, which forms by far the greater portion of the underlying surface rock. The Lower Devonian is not apparently represented in these counties, although numerous fragments of the Oriskany sandstone are scattered here and there as angular and lately detached erratics. The rocks of the Lower Helderberg group are represented only by the Tentaculite limestone or so-called water-lime beds. These latter are met with in two distinct exposures, each of which presents similar lithological characters. The Onondaga salt group or Salina formation is found to extend under the whole district, so far as can be ascertained by borings, forming the foundation rock, so to speak, of the water-lime group, and when this is absent, immediately underlying the Corniferous and Onondaga limestones. The Guelph formation—the uppermost subdivision of the Middle Silurian series, is only observed by artificial borings at the depth of about 1,150 feet from the surface of the ground, and underlying the most recent deposits of rock salt. Of the presence of the Niagara, Clinton, and Medina formations, we have but very doubtful evidence; and it is only by means of specimens of rock brought up by the sand pump, during the operation of boring, that we arrive at the probability of their existence within the average depth of 1,200 feet from the surface.

Living in the center of this salt region, I have been enabled to make frequent visits to the various salt-wells during the operation of drilling, to collect the detailed "logs" or records of each boring, and to arrive at some very important generalizations regarding the distribution and volumes of the underlying Upper Silurian and Devonian series of rocks in this locality. In order to see at a glance the character and geological sequence of the strata, it has been deemed advisable to furnish the records in full, thereby bringing to light many facts replete with novel interest and geological phenomena

The following is a list, proceeding from north to south, of the principal wells sunk and still in practical operation:—

1. Kincardine well.
2. The Ainleyville well.
3. Goderich Company's well.
4. The Dominion well.
5. Hawley's well, Goderich.
6. Clinton well.
7. Stapleton well.
8. Coleman and Gowinlock's well, Seaforth.

I. *The Ainleyville well.*

	Feet.
(1.) Sand, gravel, with boulders of gneiss and granite,	16
(2.) Gray and blue limestones; the uppermost 100 feet probably belonging to the Corniferous formation, whilst the remaining 266 feet are magnesian, and evidently belong to the Salina,	366
(3.) Layers of chert interstratified with bands of limestone,	180
(4.) Beds of compact *steatite* or *soapstone*, alternating with layers of magnesian limestone, with disseminated crystals of bitter spar,	353
(5.) Gray magnesian limestone,	97
(6.) Magnesian limestone containing traces of brine,	168
(7.) Dark-brown porous sandstones,	64
Total depth,	1244

At this depth the well was abandoned. Saline waters were met with at the depth of 1,012 feet, and were probably derived from the saliferous stratum lying further south. The position of the boring seems to mark the northeastern margin of this ancient salt lake, since the geological horizon of the salt was passed without the least evidence of its occurrence. The 97 feet of gray magnesian limestone seemingly belong to the base of the Onondaga formation, below which no brines of any economical value have yet been found. At the depth of about 1,200 feet a small water-course was met with, in which were observed traces of petroleum and bubbles of vicious gas. The brine extracted from this well was obtained only at intervals for fifteen feet, having been first observed at the depth of 1,006 feet from the surface. Specimens at no time marked over 30° salometer; they gave a specific gravity of 1·054, and consequently contained only 7·71 per cent of pure salt.*

2. *Kincardine well.*

	Feet.
(1.) Passed through the Corniferous limestone, the Tentaculite limestone, to the base of the Salina formation (the records imperfect),	883
(2.) Pure rock salt,	27
(3.) Magnesian limestone,	30
(4.) Pure rock salt,	17
Total depth,	957

Here the occurrence of a second layer of salt, separated from the first by 30 feet of limestone, leads to important considerations regarding the probability of its extension *at all parts* under the first. Indeed, to restrict the presence of this second deposit to very narrow limits within a salt-area comparatively wide, would be not only to controvert the laws regulating the distri-

* For a table giving the strength of brines from zero to saturation, see Prof. Alexander Winchell's Report on the Geology of Michigan, 1861.

bution of sedimentary beds, but also to render obsolete and void all the known theories connected with salt deposits generally. On consideration of the oscillations of level necessary for the deposition of a vast bed of salt, with the evidence of a second saliferous layer in one perforation only, the writer maintains that, within the limits of this salt-district of Ontario, a second saliferous deposit exists universally, at slightly variable distances below the first, except, probably, in the neighborhood of the margin of this ancient geographical depression.

3. *Goderich Company's well.*

	Feet.
(1.) Sand, gravel, and boulders,	30
(2.) Soft arenaceous limestone, with a layer of calcspar,	266
(3.) Hard gray sandstone, with slight traces of salt and petroleum,	78
(4.) Blue magnesian limestones,	330
(5.) Magnesian limestone, holding numerous crystals of calcspar,	110
(6.) Porous limestone, gypsum, and alternating bands of red marl and salt,	163
(7.) Rock salt,	45
Total depth,	1022

In the above well boring, commenced on the 17th Nov., 1865, and at the expiration of exactly 102 days, the salt rock was reached at about 1,000 feet from the surface. From this depth there was obtained, by pumping, a saturated brine, from which large quantities of salt continue to be manufactured. The salt-bearing stratum lies immediately at the base of the Onondaga formation, and is at once recognized by the presence of saliferous and gypsiferous magnesian marls lying as a general rule above the salt bed.

4. *The Dominion well.*

	Feet.
(1.) White and blue clays, holding boulders of Huronian and Laurentian origin,	97
(2.) Water-lime beds (Tentaculite limestone),	48
(3.) Soft arenaceous limestones,	362
(4.) Hard magnesian limestones,	331
(5.) Very hard dolomitic limestones, holding crystals of melanterite (sulphate of iron),	87
(6.) Limestone and shale in alternate layers,	120
(7.) Compact limestone and gypsiferous shales,	47
(8.) Rock salt,	21
Total depth,	1113

After boring through 21 feet of pure rock salt, the underlying limestone was reached, and at this depth the boring ceased. The *Corniferous* limestone is here absent; the first strata reached

having the character of the so-called *water-lime* beds. As shown in the record, we have for the entire thickness of the Onondaga formation at this particular locality 968 feet, of which the upper 807 are chiefly magnesian limestones, with occasional cherty layers, the underlying 161 feet being represented by gypsiferous and saliferous shales, including the mass of rock salt at the base. The brine pumped up constantly marks 87° salometer, and has a specific gravity of 1·175 at the temperature 62° F.

5. *Hawley's well, Goderich.*

The record of this well was essentially the same as that of the "Dominion," until the salt deposit was reached at the depth of 967 feet, after which the drilling was carried (1) through 12 feet of impure salt and shale, and (2) through 17 feet of pure rock salt.

6. *The Clinton well.*

	Feet.
(1.) Clay, gravel, sand, and boulders,	70
(2.) Gray cherty non-magnesian limestones (Corniferous),	108
(3.) Water-lime beds (Tentaculite limestone),	24
(4.) Hard magnesian limestones, with intercalated beds of chert,	283
(5.) Hard arenaceous limestones, with beds of shale and gypsum,	470
(6.) Coarse limestones and gypsiferous shales, with a mud-vein 3 inches in thickness,	147
(7.) Very porous limestone, containing salt,	14
(8.) Rock salt,	20
Total depth,	1136

Nothing of particular interest marked the process of boring in the above well. The presence of the *Corniferous* formation was at once detected by the borers, constituting 108 feet of the surface rock. The underlying 938 feet constitute the Salina formation (Onondaga), which here does not attain such a thickness as at Goderich, the upper portion probably having been removed by erosion prior to the deposition of the Corniferous beds. Hydrated calcium sulphate or gypsum was met with about the center of the Salina formation, occurring in compact snow-white masses along with crystals of selenite (a lamellar form of the same).

7. *Stapleton well.*

	Feet.
(1.) Boring commenced 40 feet below the general level of the country,	40
(2.) Sand, gravel, and boulder clay,	67
(3.) Light-gray limestone, intercalated with numerous mud-veins,	413

(4.)	Chert or siliceous stone, containing variable quantities of magnesia,	204
(5.)	Stratified limestone, uppermost 4 feet tolerably pure, the rest containing variable quantities of silica and magnesia. From 780 to 810 feet from the surface the rock approaches true dolomite,	176
(6.)	Shales intercalated with thin beds of clay,	80
(7.)	Crystalline brown limestone,	25
(8.)	Brown and white magnesian limestone, alternating with beds of shale and gypsum,	115
(9.)	Blue clay intercalated with gypsum,	45
(10.)	Cellular limestone, shale, and gypsum,	26
(11.)	Rock salt,	15
(12.)	Shale, gypsum, and rock salt,	14
	Total depth,	1220

This well is situated on lot 39, first concession, in the township of Hullet, 13½ miles to the southeast of Goderich, and on the line of the Buffalo and Lake Huron railway. It was sunk in 1867 by Mr. Ransford, the proprietor, and still continues to yield brines of great strength and purity, the proportion of earthy chlorides being comparatively small. The uppermost 200 feet of rock were found to belong to the Corniferous formation, which is here again observed covering the summit of the Salina group. Water-fissures were met with at the respective depths of 161 and 466 feet; but there was no evidence of the present existence of water in them. Crystals of calcspar occurred at the depth of 400 feet, and at 780 feet crystals of selenite ($CaSO^4+2H^2O$). At 952 feet a bed of compact gypsum several feet in thickness was encountered, and at 1,005 feet a layer of pure alabaster.

Before reaching the salt horizon a sudden transition from fresh water to strong brine was observed, at about 1,100 feet from the surface. Such an occurrence may be explained by the hypothesis, that an impermeable argillaceous shale completely excluded the fresh water of the upper layers of limestone from the lower saliferous rocks.

Finally, it may be mentioned that the prevalence of vast quantities of gypsum and salt in a mixed state naturally suggests the utility of a shaft, by which not only could pure rock salt be obtained, but also the combined gypsum and salt for agricultural purposes.

8. *Coleman and Gowinlock's well, Seaforth.*

		Feet.
(1.)	Gravel, sand, and clay,	25
(2.)	Stratified dark-gray limestone,	400
(3.)	Stratified magnesian limestone, followed by a very hard layer of chert,	200

(4.) Crystalline siliceous limestone, containing magnesia,	110
(5.) Blue clay, shale, and limestone,	250
(6.) Gypsum, shale, and salt,	50
(7.) Rock salt,	100
Total depth,	1135

The drilling done in this well was unprecedented in the annals of this system of mining, both for speed and absence of mishaps. Actual boring commenced on the 10th of March, 1870, and the salt-bearing stratum was reached on the eve of the 22d of the same month. After passing through 100 feet of pure rock salt, without the least evidence of change, the boring was abandoned. The great success attending this boring led to the sinking of two other wells, viz:

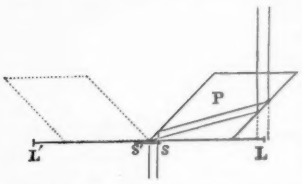
Sparling and Merchant's, in the immediate vicinity; both, however, giving records similar to the above.

Truly in no other portion of the American continent has there been discovered a deposit of salt so magnificently great. The supply is practically illimitable, and may favorably compare with the production of the salt mines of Droitwich, in Central England, or with that of the solid salt-hills of Cordova. In a future paper I shall take occasion to describe the different *systems of manufacture* at present pursued in Western Ontario.

ART. XLII.—*Comparison of the Spectra of the Limb and of the Center of the Sun, made at the Sheffield Scientific School;* by CHAS. S. HASTINGS.

A COMPARISON of the spectrum of the edge of the sun with that of its center is of great theoretical interest; but any comparison other than by direct juxtaposition must be very unsatisfactory, and the more so as the differences are less. In order to obtain spectra of two different portions of the sun side by side, where the slightest variations may be detected, I have constructed a small prism with four polished sides, its bases being parallelograms. This is so placed that one face rests upon the slit plate of the telespectroscope, and has its acute edge perpendicular to the slit at its middle point. The instrument may then be directed so that the image of the sun falls with its center on the uncovered portion of the slit, while the light which forms the edge of the sun, falling perpendicularly upon the first surface of the prism, suffers two interior total reflections and a displacement depending upon the form of the prism. A glance at the

figure, in which ss' is the slit, L L′ the diameter of the sun's image, and P the prism, shews that no light from the covered part of the slit will reach the collimating lens except that which has been reflected from the two sides of the prism. The relation of the acute angle (v) and the distance between the reflecting sides (t) to the focal length of the great telescope (F) and the width of the spectrum (a) is given by the formula,



$$2t \sin v = \text{F} \tan 16' — a.$$

The sides of the prism not fixed by the equation admit of considerable latitude, but should be made to approach the lower limit in order that the planes of the direct and transmitted images may be as little separated as possible. Of course t and v should be so proportioned that the reflections may be total.

The instruments with which the following observations have been made are those belonging to the observatory of the Sheffield Scientific School, consisting of an equatorial telescope of 9 in. aperture, and 118 in. focal length, by Clark, and a spectroscope of Young's form by the same maker. The spectroscope has a dispersive power of 12 prisms of 60°. In most of these observations an eye-piece of high power has been adapted to it, which gives a separation of the D lines equal to 64 minutes nearly. In the small prism placed before the slit, a is equal to .04 in., a quarter of the length of the slit.

When the instrument is properly directed and in adjustment, we see a very narrow black line dividing the spectrum longitudinally into two parts of widely different intensity; the fainter, belonging to the limb of the sun, is marked on its edge by the bright chromosphere lines. Upon comparing these two spectra, certain differences are recognized besides that of intensity, by far the most marked of which are exhibited by the lines b_1 and b_2, which become sharper and less hazy near the limb. The lines b_3 possesses the same characteristic, but to a less degree; C and F also become sharper in the same region. Excepting these and the D lines it requires very close examination to detect any variation. There is, however, a line in the red at 768·1 of Kirchoff's scale which is strongly marked near the center of the sun's disk, but disappears entirely, to my power at least, within 16″ to 20″ from the limb. Two other lines below F, at 1828·6 and 1830·9 of the same scale, exhibit nearly complementary phenomena, i. e., they are strongly marked near the edge, but much fainter at the center. These latter lines also become greatly strengthened over the penumbræ of spots. The line 768·1 is not thus affected. These are

all the differences which I have invariably seen in repeated examinations since the 17th of February.

Others have, however, been suspected. Certain lines, which are strengthened in a region of spots like those above mentioned, appear to be strengthened also near the edge, but do not undergo so marked a change. It is obvious that the differences should be most pronounced in the clearest sky, and such is the case. The closest examination has extended only from B to a short distance above F, as the plate glass of which the small prism is made has a decided yellow tint and absorbs the blue rays strongly.

Since the light from the border of the sun undergoes a general absorption, which reduces its intensity to much less than one-fourth that at the center, according to Secchi's measurements, and yet the spectroscopic character is changed so slightly, it is impossible for me to escape the conviction that the seat of the selective absorption, which produces the Fraunhofer lines, is below the envelope which exerts the general absorption. But the phenomena of the faculæ prove not only that this envelope rests upon the photosphere, but also that it is very thin. The origin of the Fraunhofer lines, then, must be in the photosphere itself, which is in accordance with Lockyer's views.

Any effects which the chromosphere might produce, we would anticipate finding most evident in the lines of those gases which are readily detected there. A reference to the observations shows at once a compliance with this anticipation in the lines of hydrogen, magnesium and sodium. The line 768·1 is not less strikingly in concordance, if it be regarded as 768·?* (the ? indicates doubt as to the tenths of the scale and * absence of a corresponding black line) of Young's Catalogue of Chromosphere lines. The lines 1828·6 and 1830·9, with others of the same class, probably have their origin in the medium which exerts the general absorption, and thus are allied to our telluric lines. It also seems probable that the chromosphere is too transparent to reverse many of its lines. That this is the case in the helium lines is tolerably certain.

In the apparatus described, two similar prisms were also placed over the slit in a symmetrical position. The spectra of two opposite edges of the sun were thus brought together, and the change in refrangibility due to the sun's rotation was very clearly shown.

New Haven, April 3d, 1873.

ART. XLIII.—*Contributions from the Physical Laboratory of Harvard College.*—No. IV. *Induced currents and derived circuits;* by JOHN TROWBRIDGE.

THE expression for the intensity of an induced current, deduced by Neumann and Sir William Thomson, is as follows: $i = \frac{1}{k}\frac{dU}{dt}$, in which k is a coefficient depending upon the resistance of the complete wire in the secondary circuit, and U is a certain "force function" which depends solely upon the form and position of the wire at any instant, and on the magnetism of the influencing body. The expression, in general language, is as follows:

"When a current is induced in a closed wire by a magnet in relative motion, the intensity of the current produced is proportional to the actual rate of variation of the "force function" by the differential coefficients of which the mutual action between the magnet and the wire would be represented if the intensity of the current in the wire were unity."

This investigation was undertaken to ascertain if the laws of derived circuits apply to the currents of induction, which are represented by equations of which the above is a type. A reflecting galvanometer of large resistance was included in the secondary circuit, and connected by copper wires of very small resistance with the coil in which the secondary currents were produced: the resistance of these wires was infinitessimal in comparison with the resistance of the galvanometer. The galvanometer was then shunted. The first two columns of the following table show that, with an inappreciable resistance outside of the galvanometer coils, the shunts made no difference in the deflection of the galvanometer needle when the shunts were not less than three ohms. Below this the current divided. The resistance of the galvanometer was 5880 ohms, and the last numbers in the second and third columns show that an equal

Exterior Resistances, in ohms.	Shunts, in ohms.	Deflections.	Exterior Resistances, in ohms.	Deflections.
0	3	210	10	210
"	4	210	20	210
"	5	210	30	210
"	6	210	40	210
"	5880	210	100	190

impulse was transmitted through both the shunt and the galvanometer, for no reason can be assigned why it should take

one course in preference to the other. Two galvanometers, therefore, of the same resistance, one forming the shunt to the other, will give the same deflection, which is equal to that given by the undivided circuit.

Resistances were then introduced in the circuit exterior to the galvanometer coils, and a shunt of 588 ohms was used.

The fifth column shows that no effect was produced by the shunt until the exterior resistance was appreciable in comparison with that of the galvanometer.

The following table exhibits the effect of resistances which were appreciable in comparison with the galvanometer resistance. The same shunt of 588 ohms was used. The second column is calculated on the assumption that $i = \frac{dU}{dt}$, (where k' is a coefficient), is equivalent to $i_1 = \frac{E}{R}$, and that the laws of Kirchoff hold. The third column is obtained from the experimental data. The fourth and fifth columns are also calculated on the assumption that $i = \frac{E}{R}$ = tangent of the deflection. Columns second and third are expressed in arbitrary scale divisions.

Exterior Resistances, in ohms.	Calculated value, of i.	Experimental value, of i.	Ratio of intensities.	Ratio of Tangents.
1500	1242	1375		
2000	1033	1055	1·06	1·03
2600	954	990	1·06	1·04
3000	767	825	1·06	1·04
3500	673	770	1·11	1·05
4000	613	660	1·05	1·03
4500	558	649	1·05	1·07
5000	514	550	1·04	1·03

It will be seen by comparison that, with large resistances exterior to the galvanometer resistance and appreciable in connection with it, the laws of the division of currents practically hold, and as the exterior resistance approaches that of the galvanometer, the coincidences with the laws is more marked.

From the above it appears that, under certain conditions, an induced current does not divide according to the laws of divided circuits, but approximates to these laws when there is a resistance exterior to the galvanometer which is appreciable in comparison with that of the galvanometer.

No. V.—*On a method of measuring induced currents;* by F. H. BIGELOW.

If a Wheatstone's bridge be formed, in which the secondary coil of the inductorium is the resistance R' to be measured, the relation between the resistances will be expressed by the proportion $R_1 : R_2 = R_3 : R_4$ where R_3 and R_4 are in a fixed ratio. By passing a current from an independent battery through the primary coil, the strength of the current in R_2 will be increased or diminished on the breaking or making of the inducing current. Therefore, to preserve equilibrium in the bridge, R_1 must be changed, and we shall have $R_1 \pm C : R_2 \pm x = R_3 : R_4$, in which x is a resistance equivalent to the effect of the induced current. Let $R_1 \pm C$ be found by trial such that the addition or subtraction of the induced current will produce equilibrium in the bridge; that is, will be such as to bring the deflection of the galvanometer to the zero of the scale. We shall then have the strength of the induced currents expressed as resistances. One advantage of this method is this, that the readings are always reduced to the same point, the zero of the scale. This method also has a wider range than that of merely taking the swing of the galvanometer needle. For currents which would throw the spot of light in a reflecting galvanometer off the scale, can be readily kept on the scale in this method by merely altering the ratio of R_3 to R_4 in the Wheatstone's bridge. Since the shunting of induced currents is accompanied with difficulties, as will be seen by the paper of Mr. Trowbridge accompanying this, this method is especially advantageous. When the bridge was set up so that the smallest variation in the resistance of the branch containing the inductorium gave the greatest variation in the current going through the galvanometer, namely, when $\frac{dS_0}{dR_3} = 0$, S_0 being the current through the galvanometer, and the resulting value of R_3 being $R_3 = \sqrt{\frac{GR_1(2B+R_1)}{G+2R_1}}$, in which G is the resistance of the galvanometer, B that of the circuit exterior to the Wheatstone's bridge, it was found that the induced currents could be measured to one hundred-thousandth of an ohm.

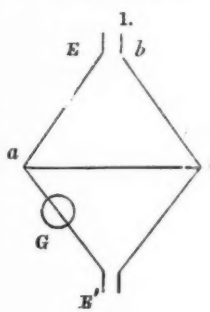
The following table contains a comparison of the induced currents produced by making and breaking the circuit. The first two columns contain the variation in ohms of the variable resistance of the bridge; the third and fourth columns give the strength of the induced currents on making and breaking expressed in ohms.

Change in the resistance on breaking.	Change in the resistance on making.	Strength of Induced Current on breaking.	Strength of Induced Current on making.
650	600	·00325	·00300
700	680	·00350	·00340
720	720	·00360	·00360
720	750	·00360	·00375
700	700	·00350	·00350
850	850	·00425	·00425

Care should be taken to send the induced currents to be compared in the same direction by means of a pole changer. It will be seen from the above table that the equality of the currents on making and breaking can readily be proved by this method.

No. VI.—*On methods of determining the resistance of a battery, deduced from Poggendorff's mode of measuring Electromotive Forces;* by N. D. C. Hodges.

In the process of obtaining the ratio between the electromotive forces of two cells, the expression $\frac{E'}{E}=\frac{B+R_0}{R_0}$ is obtained; in which $E>E'$, R_0 is the resistance necessary to bring the needle of the galvanometer G to zero, and B is the resistance of the battery to be measured. By introducing a new resistance R′ in the branch *ab*, we shall obtain the expression $\frac{E}{E'}=\frac{B+R'+R_0+R_1}{R_0+R_1}$, in which R_1 is the new value of R_0; hence $\frac{R_1}{R'+R_1}=\frac{R_0}{R+R_0}$ and $B=\frac{R_0R'}{R_1}$.



If the ratio $\frac{E}{E'}$ becomes $\frac{m}{n}$ we shall have

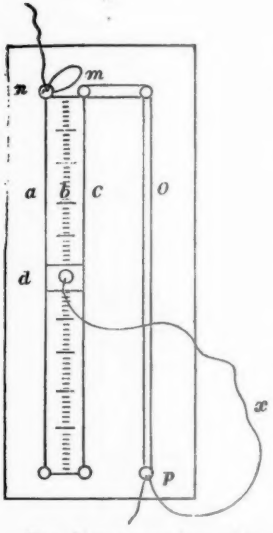
$$\frac{m}{n}=\frac{B+R_0}{R_0} \text{ or } B=R_0\left\{\frac{m}{n}-1\right\}$$

This expression was given by Mr. Mance in the Phil. Mag., vol. xli, p. 318, 1871, and is seen to follow directly from Poggendorff's method of measuring electromotive forces. The following are the results obtained from the expression $B=\frac{R_0R'}{R_1}$, in measuring the resistance of two Grove cells. One Grove cell was opposed to the two whose combined resistance was measured.

R_0	R′	R_1	B
·357	1	1·028	·347
"	2	2·057	·347
"	5	5·144	·347
"	7	7·140	·350

By Mr. Mance's method, with the same value of R_0, the value obtained was B = ·357. The above numbers are expressed in ohms and fractions of an ohm. My method has the advantage that many determinations of the battery resistance can be made with the same arrangement of cells. In Mr. Mance's method but one determination can be made without altering the arrangement. I have found the following form of Rheocord useful in these determinations.

2.



In fig. 2 *a* and *c* are two German silver wires, with a slider at *d* which has a binding screw; between the wires is a mirror *b* with a scale upon it, or at its side, in order to avoid parallax in reading. $o\rho$ is a German silver wire or rod of small resistance.

As the slider is now arranged, the current coming in at *n* will divide at *d*, pursuing the two paths $d\chi\rho$ and $dmo\rho$. By placing a box of resistance coils at χ, we can make the combined resistance of $d\chi\rho$ and $dmo\rho$ differ as little as we please from the standard resistance $o\rho$. It is thus possible, by making a large variation in χ, to measure small resistances a little larger than the small resistance $o\rho$; thus employing the correct principle of working from the greater to the less.

By disconnecting the resistance $o\rho$ at *m* and ρ and joining *mn*, it will be readily seen that the combined resistance of the circuit *dnmd* is one-half that of *nd* alone. It is often desirable to reduce the resistance in this manner.

The changes in the combined resistances can of course be readily calculated from the expression $\frac{\chi\eta}{\chi+\eta} = R$, where χ and η represent the two branches of the circuit. I have often found it advantageous in measuring large resistances to shunt them, so to speak, in the Wheatstone's balance, thus forming a combined resistance, one branch of which includes the large resistance and the other a carefully measured resistance much smaller than the resistance to be measured; the balance can in this manner be kept in its most sensitive arrangement, and a series of measurements can be made of the large resistance by varying the carefully measured portion of the combined resistance.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Action of Charcoal on Organic Nitrogen.*—Stanford has made a series of experiments to test the opinion of Stenhouse, which has since become current in science, that nitrogenous matters in contact with charcoal become oxidized to nitrous or nitric acids. Three mixtures with charcoal were made; of meat, of urine, and of excreta. Two months afterward, and every succeeding month for six months, the mixture was tested; no appreciable loss of nitrogen could be detected, nor was any nitric acid formed. Subsequently, three typical charcoals, from wood, from seaweed, and from bone, were mixed with finely chopped lean beef, equal weights of the charcoal and the meat being taken. The mixtures were placed in loosely stoppered bottles, and tested occasionally for a period of 21 months. The most marked action of the charcoal was to dry the mixtures, they losing 40 per cent of water in three months. No trace of nitric or nitrous acid could be detected throughout the experiments. Moreover, no difference was observed between bone and wood charcoal, though the one contains nine and the other 92 per cent of carbon; which could hardly be the case did carbon act as an oxidizing agent. A slight loss of nitrogen occurred, apparently as ammonia. The author concludes: (1) Charcoal, when thus applied, acts simply as a drier; (2) it does not favor oxidation and the production of nitrates; (3) after a long time and if artificially dried, the mixture may lose a little nitrogen as ammonia; (4) this loss for all practical purposes, is inconsiderable.—*J. Chem. Soc.*, II, xi, 14, Jan., 1873. G. F. B.

2. *On the Specific Heat of Occluded Hydrogen.*—Graham concluded, from his experiments on palladium when saturated by hydrogen, that these two substances formed an alloy. If this be so, then, since the specific heats of alloys approach closely the mean specific heat of their constituents, it ought to be possible to obtain the specific heat of the hydrogen when in this condition. Roberts and Wright have undertaken the solution of this problem. Pure palladium wire, also wire of palladium and 20 per cent of gold, were electrolytically charged with hydrogen, then washed with water and alcohol, heated to 100° C., and then plunged into a calorimeter, every precaution being taken to guard against error. In some of the experiments the palladium was charged to saturation, in others only partially charged, and in still others it was saturated and a part of the charge was expelled before making the determination. After the measurement, the hydrogen was expelled by heating the wire in the vacuum of a Sprengel pump, and was measured. The specific heat of palladium alone was found to be between 15° and 100°, 0·05964, increasing to 0·06022 after four times charging. That of the

occluded hydrogen varied from 4·06, where 5·4 grams of hydrogen were contained in 1000 of palladium, to 9·10, where 1·45 grams were thus occluded. The results obtained with the palladium-gold wire, corroborate those given by the palladium alone. The authors attribute the variations in the results to the irregular distribution of the hydrogen through the palladium. Moreover, since the specific heat of bodies when liquid is higher than when solid or gaseous, it might be supposed, inasmuch as the highest specific heats are found where the charge of hydrogen is lowest, that the hydrogen first absorbed is in a quasi-liquid condition, and that taken up subsequently quasi-gaseous. If so, then as the passage from the one to the other is uniform, it would indicate a continuity between the quasi-liquid and quasi-gaseous states of matter. Or, on the other hand, Graham's supposition is untenable, and the substance is not an alloy, but approaches more the ordinary solution of a gas in a liquid. Moreover, since the specific heat does not vary directly with the quantity of hydrogen, it cannot be a mixture of a definite palladium hydride with an excess of palladium. It follows, therefore, "that each several charge must be regarded as giving rise to a distinct compound, and, therefore, that palladium and hydrogen, like hydrogen chloride and water, are capable of entering into combinations . . . in proportions which are *not* expressible by simple formulæ."—*J. Chem. Soc.*, II, xi, 112, Feb., 1873. G. F. B.

3. *On the Purple of Cassius.*—The purple precipitate obtained when a dilute solution of gold is treated with a mixture of stannic and stannous chlorides, is generally regarded as a compound, probably of stannous and aurous oxides. DEBRAY has investigated this question, and is disposed to regard the purple of Cassius as a stannic (or metastannic) lake, colored by finely divided gold. If a mixture of solutions of stannic chloride and sodium acetate be boiled, stannic oxide is precipitated. If now a little gold chloride solution be added to the hot liquid, and then potassium oxalate, the gold is at once reduced, a small quantity is deposited upon the glass, but the larger quantity upon the tin, producing the ordinary purple of Cassius. Moreover, a similar substance may be obtained in a similar way with alumina. To a solution of gold chloride saturated with sodium acetate, gelatinous alumina is added and the mixture is heated; the addition of a little potassium oxalate reduces the gold and produces the purple lake. Both these lakes, suspended in water, and agitated for many hours with mercury, retain their color perfectly. The solubility of the purple of Cassius in ammonia is explained by the fact that stannic oxide is also so soluble when moist; the same influences injuring this solubility in the one case as in the other. This solution of the purple of Cassius when made is turbid by reflected light and slowly deposits metallic gold; a fact impossible to explain on the old theory. As to the purple insoluble in ammonia, obtained by the assayers in dissolving in nitric acid, silver containing a little tin, and gold, Debray shows that if the solution be

effected hot, the stannic oxide and the lake resulting are both insoluble; while if the heat used be very moderate, a purple of Cassius is obtained in this way entirely soluble in ammonia.—*Moniteur Scientifique*, III, ii, No. 372, 1007, Dec., 1872. G. F. B.

4. *On the Determination of Free Oxygen in Solution.*—SCHÜTZENBERGER and GÉRARDIN have made some experiments upon the determination of free oxygen by titrition with a graduated solution of the new sodium hydro- (or more properly, hypo-) sulphite, discovered by the former, which, as is well known, has a strong attraction for oxygen. In presence of free oxygen, it becomes sulphite at once:

$$HNaSO_2 + O = HNaSO_3.$$

A half hour before the determination, a flask of 60 to 100 c.c. capacity is filled three-quarters full with water, a spiral of leaf zinc is placed in it, and to it is added 10 c.c. of a solution of hydrosodium sulphite of 20° B. It is then filled with water and closely stoppered. By occasional agitation the reduction is effected in 20 to 25 minutes. It is graduated by placing in a test glass 20 c.c. of a solution of copper (copper sulphate in ammonia, 10 c.c. of which, in passing from cupric to cuprous oxide, lose 1 c.c. of oxygen) covering this with a layer of oil. By the side of this is placed a bottle containing a liter of the solution to be tested, also covered with oil, and previously colored blue with Coupier's blue (which is decolorized at once by the hyposulphite, while the sulphite does not affect it) used as an indicator. Into a burette of 50 to 60 c.c. capacity divided into tenths c.c. the hyposulphite solution is placed. Its titer is first ascertained by noting the amount necessary to decolorize the copper solution. Then at once and from the same burette, the solution is added to the liquid to be examined until it is decolorized. During these operations, the opening of the burette is kept below the level of the oil. By a simple calculation, the amount of dissolved oxygen is easily ascertained.—*Moniteur Scientifique*, III, iii, No. 373, January, 1873. G. F. B.

5. *On Coloring Matters from the Aromatic Azodiamines.*—HOFMANN, having studied three years ago Magdala red, a color produced by the action of naphtytamine upon azodinaphtyldiamine, has lately, in connection with GEYGER, undertaken to study the results of this reaction in other cases. They find that both aniline and toluidine, heated with azodiphenyldiamine in a sealed tube to 160° for four or five hours, produce a blue coloring matter, which they call *azodiphenyl blue.* The reaction is:

$$C_{12}H_{11}N_3 + C_6H_7N = C_{18}H_{15}N_3 + NH_3.$$

This blue is a strong base, dark brown in color, insoluble in water, soluble in alcohol and ether, and colored blue by acids. Heated in alcoholic solution with hydrochloric acid and zinc turnings, it yields a leucobase. The aniline-violet obtained by Girard, DeLaire and Chapoteant, by the oxidation of aniline, has also the empirical composition $C_{18}H_{15}N_3$; but the authors were not able

to satisfy themselves that they are identical. They also examined *safranine*, an orange coloring matter, formed by the oxidation of aniline. It is a toluidine derivative, $C_{21}H_{20}N_4$, formed by the union of three molecules of the monamine, with the substitution of one atom of nitrogen for three of hydrogen, and the elimination of four atoms of hydrogen.—*Ber. Berl. Chem. Ges.*, v, 472, 526. 1872. G. F. B.

6. *Sensibility of the Eye in determining differences in intensity of different colors.*—HELMHOLTZ gives to the Academy of Berlin the following analysis of the work of W. Dobrowolsky, of St. Petersburg, on the sensibility of the eye in determining differences in intensity of different colors. If two colors of the same tint and of different intensities are placed side by side, the eye appreciates their difference in intensity as long as it exceeds a certain fraction of the intensity of the light observed, as Fechner first made known. This fraction for white light is about $\frac{1}{100}$, and in certain exceptionally favorable cases it equals $\frac{1}{150}$ or $\frac{1}{180}$. The author has made researches on different regions of the solar spectrum, with the assistance of two other observers, and gives the following figures for the values of the denominator of the fraction.

First observer.

Red,	A,	14·	Blue,	*E* to *b*,	67·3
"	B,	19·76	"	F	131·6
"	C,	25·16	Indigo,	G	268
Orange,	C to D,	33·16	Violet,	G to H	268
Yellow,	D,	45·77	Violet,	H	67
Green,	D to E,	58·77			

		Second observer.	*Third observer.*
	B	15·9	11·7
	D	40·86	27·5
Between	G and H	205·5	205·5

The intensity of the violet was not sufficient to give the maximum sensibility. We see from these results that the sensibility of the eye in its appreciation of differences in the intensity of colors, increases as we go from the red to the violet. A. M. M.

7. *Galvanic reduction of Iron under the influence of a powerful electro-magnetic solenoid.*—JACOBI, of St. Petersburg, covered the interior walls of two glass vessels with cylinders of sheet-iron, and placed in these vessels two similar rods of wax, coated first with a thin electro deposit of copper, and then with plumbago. The vessels were then filled with a solution of sp. gr. 1.27, containing 135 parts of ferrous sulphate, and 123 parts of magnesium sulphate, rendered neutral by the addition of magnesium carbonate. One of these vessels was surrounded by a tube of sheet iron, in which was coiled a helix of insulated copper wire. A reducing current from one Smee cell was now passed through the solution in the two vessels, while another current from four Bunsen cells went through the magnetizing helix. At the end of twenty-eight days the wax rods were examined, with the following results: An

equal weight of iron was deposited on each rod; but, while the iron on the rod not exposed to the heliacal current was smooth and fair, the iron on the other rod was principally on its upper and lower portions in the form of tufts, having a crystalline structure, and resembling somewhat the appearance presented by a bar magnet after its introduction into iron filings.

Jacobi found that both deposits were very feebly magnetic, and further experiments showed that iron deposited by electolysis receives a remarkably high charge of temporary magnetism, and has very feeble coercive force; he therefore recommends such iron for the construction of electro-magnetic cores.—*Ann. de Ch. et de Phys.*, Feb., 1873. A. M. M.

8. *Ozone and Antozone. Their History and Nature. When, Where, Why, How is Ozone observed in the Atmosphere?* by Cornelius B. Fox, M.D., Edin., Member of the Royal College of Physicians, London, etc., etc. 330 pp., 8vo. 1873. Illustrated. London: J. & A. Churchill.— A very satisfactory account of the numerous investigations into the nature of ozone and antozone, into their natural occurrence, and the methods of identifying them, as well as of the observations that have been made upon them and the conclusions based on these observations. It is a critical account also of these several matters, not indeed, exhaustive, but very fair and just. On the whole, an excellent book, very suggestive, very compendious, well arranged, and as a guide, aid, and stimulus to new investigations, invaluable. The whole matter of the natural occurrence and offices of ozone needs renewed, comprehensive and systematic research. The conflicting conclusions and observations hitherto made reveal a most unsatisfactory state of knowledge upon a topic which is of high and universal interest. The first preliminary to the needed new investigations is just such a laborious collation of all existing work as Dr. Fox has given us. S. W. J.

9. *Elements of Natural Philosophy;* by Professors Sir W. Thomson and P. G. Tait. Part I. 279 pp. 8vo. 1873. Clarendon Press Series: Macmillan & Co.—This work consists in great part of the large type, or non-mathematical portion of the first volume of the *Treatise on Natural Philosophy* by the same writers. That volume was published in 1867, and was designed to be followed by three others. It has been for some time out of print, and we are glad to see an announcement of a second edition of it. Many persons, however, will prefer this smaller work, as the omitted mathematical portions of the *Treatise* are not always elementary. The mathematical methods employed in the *Elements* are, almost without exception, limited to those of the most elementary geometry, algebra, and trigonometry. It is designed for use in "schools and in Junior classes in Universities." The subjects treated of in this volume are Kinematics, Dynamical Laws and Principles, Experience, Measures and Instruments, Statics of a particle, and Statics of solids and fluids.

II. Geology and Natural History.

1. *Notes on the Island of Curaçao;* by W. M. Gabb. (Letter to J. D. Dana, dated Curaçao, Jan. 20, 1873.)—Curaçao is one of a series of barrier islands lying off the coast of Venezuela, at a distance of about 30 to 40 miles from the main land. It is a long barren strip, nearly 40 miles long, with a trend to the southeast. Its surface is in the main flat, and but little elevated above the sea level. The bay of Curaçao, around the mouth of which the town is built, is a long intricate body of water, divided into several arms, and, entering from the south side, almost bisects the island.

The geological structure is extremely simple. There is but one rock formation and that is what I have termed the "Coast limestone" in my memoir on Santo Domingo. (Trans. Amer. Phil. Soc.) It is lithologically identical, being an amorphous carbonate of lime, containing occasional fossil shells and corals, all of living species. Unlike the rock in Santo Domingo, for which I suggested the term *Antillite*, it shows no soft earthy or chalky character, so far as I have been able to examine the few existing excavations. But I suspect that deep quarrying would develop this "chalk," since it occurs in most if not all of the other islands, at a depth beyond the reach of atmospheric influences.

Back (north) of the town is the bed of a dry water course; a few shallow pits full of brackish water show that here as in the Bahamas, Bermudas and elsewhere, the fresh surface water exists, floating on top of the sea water, which percolates through all the cracks and cavities of the rock. (See Nelson's memoirs in the Trans. and Quart. Jour. Geol. Soc.)

The stratification of the rock is usually horizontal or nearly so; but the few hills in the vicinity of the city show that the island owes its existence to the presence of a well marked anticlinal axis. The direction of the upheaval is coincident with the trend of the island, and the dip on the back side is seaward, that is to say, the hills on the north side of the island show a northern dip, while those on the south side have the rocks dipping about 20° southward.

In the immediate vicinity of the city, a series of hills run to the westward, their northern or inland faces being quite precipitous, while the southern sides invariably have the slope of the rock strata. Some of these reach a height of 100 feet. One, immediately back of the city, I estimated to be about 130 to 150 feet high. The central line of the island is nearly flat, divided into a

a, a, sea level; B, citadel back of the town, about 130 feet high; *c, c, c,* internal rolling plain; *d, d,* table land bordering the north coast; *e, e,* high hill (100 ft.) off the line of section.

slightly rolling surface, but with an elevation so much less than the hills, that it can only be accounted for by the existence of a fault. North of this again, bordering the north coast, at least in part, is

a table land, abrupt inland and sloping slightly seaward, thus giving as a cross section of the island, in the vicinity of the city, a structure like the preceding.

The soil of the island is scanty except in the little village in the interior (*c*, *c*, of section), where it has accumulated through the aid of rains. Here fruit trees are cultivated with great care. Elsewhere the rocks are not infrequently entirely bare. There is no vegetable mould and the earth is red, the same characteristic "red earth" or clay as that found in Jamaica, Santo Domingo, the Bahamas, &c. The climate and vegetation are very similar to the rainless parts of Santo Domingo. Showers are very rare, springs, properly speaking, do not exist and the water supply is derived principally from carefully constructed cisterns; the deficiency being made up by poor water obtained from the shallow wells. The plants are of the same species, at least in part, as those found between Santiago and Monte Cristo, Santo Domingo. Two or three species of stunted acacias, or Cereus, 10 to 12 feet high, and one or two species of Opuntias are the most striking. Another noticeable feature is the large number of individuals of two species of land shells, a Pupa and a Cyclostoma, which literally swarm over the trunks of the trees and lie in little heaps around their roots. Besides these, lizards a foot and a half long, by thousands, make up the greater part of the native fauna. Very few birds are found, and I have not seen a single snake.

I have learned but little of the neighboring islands. On Little Curaçao there is a deposit of guano, now being mined on a large scale. Buenos Ayres shows a series of peaks, several hundred feet high, but I am told that they are composed of the same rock as that of Curaçao. Here also, there are guano deposits, but the guano is said to be poor in phosphoric acid. The whole region, including the adjoining parts of the main land, is dry and arid, and, as the result of careful inquiries, I am led to believe that the large peninsula adjoining the bay of Maracaibo is also of the same horizontal Post-pliocene rock.

2. *Spergen Hill fossils identified among specimens from Idaho;* by F. B. Meek. (Communicated.)—A remarkable group of very small fossils has long been known to occur at a locality called Spergen Hill, near Bloomington, Indiana, at about the horizon of the St. Louis limestone of the Lower Carboniferous series. The most singular peculiarity of these fossils is, that, although showing every evidence of being adult forms, most of them are of *very* diminutive sizes, and look like the merest miniature representations of known larger species. They belong, for the most part, to well known genera of *Corals*, *Blastoïdea*, *Brachiopoda*, *Lamellibranchiata*, *Gasteropoda*, *Cephalopoda*, &c., and are found in a very beautiful state of preservation, crowded together in immense numbers.

A few of these little fossils have been found at the same horizon in Illinois, Iowa, and Missouri, scattered through the rocks, along with larger species; but at no other locality have so many of the

species been found crowded together, in such vast members, as at Spergen Hill. Nor am I aware that any of the species have been hitherto identified from any locality west of Missouri and Iowa.

In studying last summer's collections of Dr. Hayden's party, I was much surprised to see several of these little Spergen Hill fossils, in a dark gray crumbling mass of limestone, found by Prof. Bradley, of Dr. Hayden's corps, on the divide between Ross Fork and Lincoln valley, Idaho. This mass was only about seven inches in length, by three to four inches in breadth and thickness. On breaking it to pieces, I found it to contain hundreds of little fossils, belonging to about seventeen species of the same genera found at Spergen Hill; and a careful comparison also shows that about one half of these seem to be, so far as can be seen, *specifically* undistinguishable from Spergen Hill forms.

Prof. Bradley has since broken up the fragments more carefully and found a few additional species. The following is a list of these fossils. Those preceded by an asterisk are believed to be in all respects identical with species found at Spergen Hill; while the others are at least more or less closely allied representatives of forms occurring at that locality:

Cyathaxonia, (like a Spergen Hill species.)
**Pentremites conoideus* Hall.
Pentremites, (like *P. Koninckianus* Hall.)
Rhynchonella mutata Hall?.
**Rhynchonella macra* Hall.
**Retzia Verneuiliana* Hall.
Athyris, (representing *A. Shumardi* H.)
Spirifer, (like *S. Norwoodi* H.)
**Productus biserialis* Hall.
**Nucula Shumardiana* Hall.
**Conocardium Meekianum* Hall.
**Cypricardella plicata* Hall.
**Cypricardina Indianensis*, (= *Cypricardia* H.)
**Straparollus* (*Euomphalus*) *Spergenensis* Hall.
Pleurotomaria, (undetermined sp.)
Bellerophon, (like *B. sublævis* H.)
Naticopsis, (like *N. Carleyi*, = *Natica Carleyi* H.)
Platyceras, (undt. sp.)
Cythere, (like *C. carbonaria* H.)
Phillipsia, (like a Spergen Hill sp.)

The occurrence of so many identical and representative species of such peculiar diminutive fossils, crowded together in the same way, at these two widely separated localities, is, to me, a very curious and interesting fact.

Washington, D. C., Feb., 1873.

3. *On the Probable Existence of Microscopic Diamonds, with Zircons and Topaz, in the Sands of Hydraulic Washings in California;* by Prof. B. Silliman.*—The occurrence of diamonds of some size in the gold fields of California is by no means uncommon, and was noticed by me in a communication to the California Academy of Science in 1867, when specimens of this gem, from at least five different localities, were exhibited. I then suggested that a more attentive examination of the heavy sands left in the sluices of hydraulic washings would in all probability detect diamonds, mingled with other rare species not commonly believed to occur in these sands.

Mr. George A. Treadwell, of San Francisco, has lately sent me a small package of these sands, collected by him from the sluices

* A paper read before the American Institute of Mining Engineers, Boston, Feb. 19th, 1873.

of the "Spring Valley Gravel Mining Claim," Cherokee, Butte county, California. A microscopic examination shows these sands to abound in beautiful colorless zircons (hyacinths), of the form well-known in the hyacinths of Expailly (France), associated with crystals of topaz, quartz in fragments, rounded grains of chromic and titanic iron, and a few small, almost globular, masses of very high refracting power which appeared to be diamonds. To determine this chemically, a portion of the sands was treated with acid for the removal of any carbonates which might be present. There was no effervescence from this treatment. The same sample was then digested in strong sulphuric acid of a high temperature to destroy any particles of organic matter which might be present, washed out in pure water without contact with organic matter, dried and ignited in a vessel of platinum out of contact with air. This sample of the sands thus freed from anything which could afford carbonic acid, but the diamond, was then ignited in a platinum nacelle (boat) in a tube of hard glass, and in a current of pure dry oxygen gas, which, for precaution, was passed over soda lime, and then, after passing the ignited assay, was delivered through a solution of baryta water. The transparency of this delicate test was soon disturbed, and by continuing the experiment for about an hour, a notable quantity of baryta carbonate was obtained. This experiment seems to prove that diamond powder was present in small quantity. When I am provided with a larger quantity of these sands, I propose to determine the amount of diamond dust quantitatively.

It will be remembered that Prof. Wöhler, some years since, found diamonds by a similar method in the platinum sands from Oregon, associated with the rare species *Laurite*—sulphide of osmium and ruthenium. His paper will be found in the American Supplement to the Chemical News for November, 1869, p. 317.

The black grains, which contribute fully one-half the bulk of the Butte county sands, are about equally chromic iron, which the magnet removes, and titanic iron, which is unaffected by the magnet. The chromic iron was so proved by the blowpipe, and no magnetite could be detected. No metallic grains of any of the platinum or iridosmium metals, or gold, could be found.

In his letter to me, accompanying the sample sent, Mr. Treadwell says: "I have examined much of the sand under the microscope, and think there are a few fragments of broken diamonds. These sands were taken from the tailings after passing through a long flume paved with stone. You know what sharp, hard pounding the gravel gets, mixed with boulders, in a hydraulic flume. No doubt, some diamonds are ground, or rather broken, by hard knocks to powder."—*Engineering and Mining Journal*, Feb. 25, 1873.

4. *Prof. Henry Wurtz on Metamorphism as a consequence of the transformation of motion into heat.*—Mr. Wurtz, in a letter to J. D. Dana, dated Hoboken, N. J., April 5th, mentions the fact that he presented the view that the heat of metamorphism was

due to motion in the altered strata, in a memoir read before the Buffalo meeting of the American Association, August 1st, 1866, entitled "Gold Genesis." The memoir was published in the American Journal of Mining, January 25th, 1868, under the heading "Gold-Genetic Metamorphism, with some views on Vein-Genesis," and in it occurs, besides other allusions to the subject, the following paragraph:

"There is one related point to which I shall devote a few words; as due weight may not have been attached thereto by geologists, even supposing that it has occurred to any in precisely the same light. This is, that the tremendous dynamic agencies, whose effects of upheaval, subsidence, disruption and displacement, we find so widely manifest, while doubtless themselves engendered of the pent-up heat-energy of the interior, must have given birth to, or have been in part transmuted into, heat-motion. Hence I deduce conclusions of great moment, but one or two of which can now be dwelt upon. It follows, for instance, that in our theoretical views of metamorphism, we are by no means of necessity limited, for our essential chemical excitant, merely to that portion of the hypothecated residual cosmical heat which might be supposed to have been retained by the emerging oceanic floor. Neither elevation nor subsidence (both necessarily accompanied by enormous compression) could occur without rise of temperature; though the degree of this rise would, of course, vary very much in various parts of the mass. The era of possible metamorphic changes dependent upon the percolation of superheated waters is thus indefinitely prolonged, *even to the present time:* and explanation, both in mode and measure, is thus presented for our thermal springs and many like phenomena." * * *

The foot-note indicated by the star at the close of the last paragraph begins thus: "In passing by for the moment this important subject, I will ask whether the general rise of heat represented as found on descent into European mines, may not possibly admit of a similar explanation," meaning of course heat, or *molecular* motion, derived from *molar* motion, which latter I presume means 'work.'"

In this memoir, Prof. Wurtz stated the probability, if he did not prove the necessity, of the existence of gold in solution still in our present ocean, and predicted its probable future discovery, as lately verified by Sonstadt.

5. *Pitchblende and Tellurium-Gold Ore in Colorado*—Professor N. P. Hill, writing from Blackhawk, Colorado, to one of the editors of this Journal, says: "A discovery was made some time since, about a mile from Central City (Colorado), of *pitchblende.* It is now found in large quantities, several tons of ore containing fifty per cent of uranium oxide having been shipped to England. The ore has commanded a price of $1 per pound thus far."

The same correspondent says: "The Red Cloud Mine has produced considerable quantities of an ore of tellurium rich in gold

and silver, the samples which I have seen containing also lead." Samples of both these ores have been sent us by Mr. Hill, and when they arrive we shall hope to determine the species to which the tellurium ore belongs. B. S.

6. *Contributions to a Fauna Canadensis, being an account of the animals dredged in Lake Ontario in* 1872; by H. ALLEYNE NICHOLSON. (From the Canadian Journal.)—A preliminary report of these dredgings was published in the Annals and Magazine of Natural History, vol. x, p. 270, October, 1872. The present paper includes most of the matter of the previous one, with a revised list of the species and descriptions of some that are regarded as new.

The dredgings were all in shallow water as compared with those made by Mr. S. I. Smith, in Lake Superior, in 1871.* The greater part of the species were obtained in Toronto Bay, where the depth was from one to three fathoms. Some dredgings were also made in the open lake, where the water was from 8 to 40 or 50 fathoms deep. But in most of the deeper dredgings very few animals were found. The list of animals obtained includes 43 species, of which 21 are shells, and 6 fishes and reptiles. The minute species are omitted. The shells are all inhabitants of shallow water, and most of them are species that are widely distributed in the fresh waters of the northern United States and Canada. *Valvata tricarinata* was the only species found living at depths as great as 8 fathoms; all the others were from less than 4 or 5 fathoms. Three species of leeches are described and figured as new. One of these, *Clepsine paterliformis*, appears to be perfectly identical with *C. elegans*, described by me in this Journal, vol. iii, p. 132, Feb., 1872. The color differs slightly from the variety originally described, but the color-variety that he describes is not uncommon at New Haven. Prof. Nicholson states that this species carries the young attached to the ventral surface by means of their posterior sucker, and thinks that this is a remarkable habit. He says, "This extraordinary habit of carrying the young has been noticed by Prof. Verrill, in another species of *Clepsine;* but I am not aware that attention has otherwise been drawn to it." In the paper by me, to which he refers (this Journal, Feb., 1872), I gave this habit as a *generic* character (as many other writers had done before me), saying (p. 127), "The young adhere in a group to the posterior part of the lower surface of the body of the parent, by means of the posterior sucker, and before quitting the parent, usually present the essential characters and often nearly the pattern of color of the adult, though paler." And in describing the species, the attached young of four species were mentioned, and more or less fully described.†

* See this Journal, vol. ii, pp. 373, 448, Nov., Dec., 1871; also, Preliminary Report on the Dredging in Lake Superior, by S. I. Smith, in the Report of the Chief of Engineers to the Secretary of War, vol. ii, 1871.

† I might have added that the eggs before hatching are also retained in the concavity beneath the body, in a cluster, and thus *incubated* till they hatch, the

It is strange that Dr. Nicholson should have overlooked my statements in regard to this point, except in the case of one species, especially as I had the pleasure of sending him my paper, before his first one was written; and it is equally strange that the author of a text book of Zoölogy should not have been aware that this habit has been well-known for at least fifty years, and has been described by nearly all writers upon leeches. This genus of leeches is one of the most common in Europe as well as in America, and is represented by ten or more species in England, where their habits are also well known. Johnston, in his Catalogue of British Non-parasitical Worms, p. 50, 1865, even gives this habit as a peculiarity of the *family*. One would suppose that Dr. Nicholson might have had abundant opportunities, while living in England, to have observed this habit among the English species. His *Clepsine sub-modesta* does not appear to differ from young specimens of one of the common color-varieties of my *C. modesta*. The differences in form mentioned depend merely upon the degree of activity of the animal. His *Nephelis vermiformis* is evidently young, and the description and figure are insufficient for its identification. No characters are given sufficient to separate it from the young of the common *N. lateralis* (Say), or my *N. fervida* from L. Superior. In color it agrees best with the latter.

His *Sænuris Canadensis*, if correctly described, does not belong to the genus *Sænuris*, for in the latter the setæ are never *forked*. This character would throw it into another family, even, —near *Lumbriculus*. The same remark applies also to the next species, indicated as "*Sænuris* sp." His next one, "*Lumbriculus* sp." is described as having "four rows of straight spine-like setæ, arranged in pairs." This would therefore not belong to the same family with *Lumbriculus*, in which the setæ are forked and in fan-shaped fascicles; it probably belongs to *Lumbricus*, or some of the closely related genera.

The only crustacean that the author attempts to name specifically is *Pontoporeia affinis* Lindström, which Mr. S. I. Smith found abundantly in Lake Superior in 1871, and identified *by direct comparison with type specimens, from the Swedish lakes*, sent by Dr. Lovèn (see this Journal, vol. ii, Dec., 1871, and Preliminary Report on Dredging in Lake Superior, Oct., 1871). The crustacean obtained by Dr. Nicholson may, perhaps, be this species, but he does not seem to be quite sure of it. Concerning the other species of Amphipods referred more or less doubtfully to *Gammarus* and *Crangonyx*, it is impossible to say anything, as no characters are given sufficient to indicate the genera, and the members of this group are notoriously difficult to determine without the most careful study. The *Mysis relicta* Lovèn,

leech generally remaining stationary in one place during this period, with that portion of the body that covers the eggs arched, and moving constantly with an undulatory motion, evidently intended to force a constant current of water over the eggs.

found in abundance by Dr. Stimpson and others in L. Michigan,* and by Mr. Smith in L. Superior, and first identified by Mr. Smith, with the species from the Swedish Lakes, by comparison with the specimens sent by Lovèn, was not found by Dr. Nicholson. And although he mentions the occurrence of the *Mysis* and *Pontoporeia* in Lakes Superior and Michigan, and alludes to the great importance of this interesting discovery, he does not mention, in his former paper, nor in the present one, *by whom* these discoveries were made. He in no case gives any credit to Mr. Smith, by whom these species were first recognized as American, and who also fully appreciated the importance of the fact of their identity with the Swedish species. Nor does he mention Mr. Smith's name in connection with the dredgings in Lake Superior, although his researches were of far greater importance, and on a much larger scale than those of Professor Nicholson, extending as they did to the greatest depths of Lake Superior (169 fathoms) and crossing the lake in various directions. Moreover, the two papers giving the results of Mr. Smith's researches had been sent to him, before he undertook his dredging operations, by Mr. Smith, at his request, so that he could not have been ignorant of them.

Some suitable acknowledgement of the labors of Mr. Smith in dredging so extensively, and so promptly publishing the results of his researches, was in justice demanded of Mr. Nicholson, who has evidently derived some benefit from them. Not to mention his name at all in this connection was a kind of discourtesy which, fortunately, is rare among American naturalists of repute.

A. E. V.

7. *Tieghem on the Cotyledon of Gramineæ, etc.*—The old controversy between Richard and Mirbel, early in the century, respecting the structure of the embryo in grasses, was never settled. Reduced to modern morphological terms, the views maintained amount to three. According to the first, the *scutellum* or outermost piece of this complex embryo is the cotyledon, the lobule opposite (which is not always present) represents the second leaf, the *pileolus*, or conical tunic which envelops the parts of the plumule that develop into foliage, is the third leaf, so that the earliest green leaf is the fourth of the series. Under the second view, the scutellum is also the cotyledon, but the opposing

* A brief account of the first dredging expedition (1870) in L. Michigan, has been published in the Trans. of the Wisconsin Academy of Science, etc., 1872, p. 98, by Dr. P. R. Hoy, who was one of the party and the original discoverer of the *Mysis* and other deep-water species found in the stomach of white-fish (*Coregonus*). His paper has been reprinted in the Annals and Magazine of Nat. Hist., April, 1873, p. 319. Another exploration was carried on in 1871, but the collections of both these expeditions were burned in the Chicago fire. A set of the Crustacea from the former was, however, given to Mr. S. I. Smith, just before the fire, by Dr. Stimpson, with his MSS. names attached. These names were given only provisionally, and without much examination, and should not have been published; but as they have appeared in Dr. Hoy's paper and its reprint, it may be well for me to state here, on the authority of Mr. Smith, who has compared the original specimens, that *Gammarus brevistilus* Stimp. MSS. is *Pontoporeia affinis*, and *Mysis diluvianus* Stimp. MSS. is the same as *Mysis relicta* Lovèn.

lobule a mere appendage of it, the rest as in the first view. According to the third view, the scutellum, with its lobule where that distinctly exists, is an unusual expansion of the axis (caulicle, tigelle, or radicle) below the cotyledon, the pileolus is the cotyledon enrolled and sheathing, and the first green leaf is the second of the proper series. Van Tieghem, in Ann. Sci. Nat., sér. v, 15, has taken up the investigation anatomically, as is his wont, following the clue of the vascular threads (the libero-vascular bundles). He determines that the scutellum is the main body of the cotyledon (it receives vascular bundles, one or more, from the axis as does a cotyledon or leaf, but as any mere enlargement or lateral expansion of axis should not); that the *lobule* (which gets no vascular element) is a mere appendage of the former; and that the pileolus belongs to the cotyledon in a different sense, and is the analogue of the ligule or stipule, and derives its vascular elements, in the form of two bundles, from the scutellum. There are homologous cases in the monocotyledons, as in Canna. But *Gramineæ* and *Cyperaceæ* are remarkable for a greater specialization of parts, and for a profound separation between the part which answers to the lamina of the leaf, the *scutellum*, and the part which represents the "superior sheath," ligule, or stipule, which in this instance takes an extraordinary and independent development. A. G.

8. "*Infelix Lolium.*"—*Darnel*, it would appear, has got a bad name without deserving it. It has long been counted as the exception in its order, "the only authentic instance of unwholesome qualities in the order of grasses." The authenticity of the exception has recently been tested in Scotland, by Mr. A. S. Wilson, who reported the result of his trials to the January meeting of the Edinburgh Botanical Society. As Lindley, in his Medical and Economical Botany, says: "Grains narcotic and acrid, producing fatal consequences when mixed with flour," Mr. Wilson began by taking small quantities of the meal of Darnel, raised by himself, rising from two grains (8 kernels) to fifty grains, then eating a mess of pottage made of a hundred grains, and finally eating cakes made in great part of the husks or bran of Darnel. No symptoms of any kind were experienced. A. G.

9. *Saccardo on certain small Bodies in the Fovilla of Pollen* (Nuovo Giornale Botan. Ital., Dec. 10, 1872.—The author states that botanists are agreed that the minute grains in the contents of pollen consist of starch granules, oil-globules, sugar, and nitrogenized compounds but, so far as he is aware, no observer has yet noticed among them certain minute bodies of well-marked and constant shapes. He detected, in June last, very small oscillating bodies which make up the bulk of the fovilla, and to these he gives the name *Somatia*. The form of the somatia is invariable in the same species of plants, and in plants of the same genus the forms appear to be nearly identical. The plants most carefully studied were *Cucurbita Pepo*, *Eschscholtzia crocata*, *Onagraceæ*, *Portulaca grandiflora*, *Althæa rosea*, whose *somatia* are figured as fusiform, discoid, &c. To observe these small bodies to

the best advantage the author advises that a drop of distilled water should be placed on a few grains of the pollen on a slide, and then the cover should be pressed down so as to crush them. The somatia are seen under a magnifying power of 800 to 1000 diameters to have an oscillating motion which may be referred to the "Brownian movement." Treated with a solution of iodine, the color of these somatia becomes blue; but this tint is marked only in the central portion, while the outer part remains clear. The author does not venture to give any theory in regard to the office of the somatia. G. L. G.

10. *A Contribution to the History of the Fresh-water Algæ of North America*, by HORATIO C. WOOD, Jr., M.D., etc. Washington, January, 1873; published by the Smithsonian Institution.—It is now fully twenty years since the Smithsonian Institution performed an appropriate and most acceptable service by publishing the *Nereis Boreali-Americana* of the lamented Prof. Harvey, thus enabling our students to study the marine *Algæ* of our coasts. It proved to be one of the most popular of the Smithsonian Contributions to Knowledge. The institution has now enabled our students, and all who are curious in microscopic life, to enter upon the more difficult but not less interesting investigation of the Fresh-water Algæ, by bringing out Prof. Wood's important contribution. The systematic part of this goodly volume consists of 239 pages, of imperial quarto size, in which all the United States species known to the author (exclusive of *Diatomaceæ*) are arranged and described; they are illustrated by twenty-one colored lithographic plates, which appear to be excellent. A supplement contains six species, which are described in Prof. Harvey's Nereis, which Prof. Wood did not consult in season to include in their proper places; and in the preface a fine list of fresh-water species collected by Mr. Olney in Rhode Island and named a long time ago by Prof. Harvey, is reprinted from Mr. Olney's *Algæ Rhodiaceæ*. Any student of these interesting forms may thus infer how much remains to be known of them, and all should unite in thanking Prof. Wood in thus opening the way to their investigation; also for the elaborate bibliography appended to the volume. This fills thirteen pages in double columns, and is an almost exhaustive enumeration of the works and scattered papers which relate to this group of plants. A. G.

11. *Botanical Necrology*, 1872–3.—Our record begins and closes with the names of eminent botanists who have been taken from our own home circle.—The following brief biographical notice of the Rev. Dr. Curtis is taken from the Council's Report to the American Academy of Arts and Sciences (Proceedings, vol. viii, p. 471):—

MOSES ASHLEY CURTIS was born in Stockbridge, Massachusetts, on the 11th of May, 1808. His father was the Rev. Jared Curtis, of Stockbridge, afterward for many years chaplain of the State Prison at Charlestown. His mother was a daughter of General Moses Ashley. He was fitted for college chiefly under his father's

tuition, and was graduated at Williams in the class of 1827. Three years afterward he went to Wilmington, North Carolina, as a tutor in the family of Governor Dudley, while at the same time he studied divinity. There he resided until the year 1841, with the exception of a year and a half passed with his father in Charlestown. In the autumn of 1834, he married Miss De Rosset, of Wilmington, who survives him. He took holy orders at Richmond, Virginia, in the summer of 1835; became rector of the Protestant Episcopal Church at Hillsborough, North Carolina, in 1841, and fulfilled the duties of this station for the remainder of his life, with the exception of ten years, from 1847 to 1857, during which he had the pastoral charge of a parish at Society Hill, South Carolina. The degree of Doctor of Divinity was conferred on him by the University of North Carolina, at Chapel Hill. His health for a few years past was sensibly impaired; but he was able to perform his professional duties, and, in a measure, to prosecute his scientific studies, until the 10th of April last, when he died suddenly, probably of heart disease.

Dr. Curtis's attention must have early been attracted to botany, and his predilection fixed by his residence at Wilmington, one of the richest and most remarkable botanical stations in the United States. For it was in the year 1834, after only three years' residence there, that he communicated to the Boston Society of Natural History his first botanical work, namely, his "Enumeration of Plants growing spontaneously around Wilmington, North Carolina, with remarks on some New and Obscure Species." This was printed in the first volume and second number of that Society's Journal; but the original impression having been mainly destroyed by fire, important additions and emendations were made in the subsequent reprint. The author's powers of observation and aptitude for research are well shown in this publication, and it is one of the first of the kind in this country in which the names are accented. In his note upon the structure of Dionæa, or Venus's Fly-trap,—a plant found only in the district around Wilmington,—Dr. Curtis corrected the account of the mode of its wonderful action which had prevailed since the time of Linnæus, and confirmed the statement and inferences of the first scientific describer, Ellis, namely, that this plant not only captures insects, but consumes them, enveloping them in a mucilaginous fluid which appears to act as a solvent. Extending his botanical observations to the western borders of his adopted State, Dr. Curtis was among the first to retrace the steps and rediscover the plants found and published by the elder Michaux, in the higher Alleghany mountains. But for the last twenty-five years, his scientific studies were mainly given to mycology, in which he became a proficient and the highest American authority. His papers upon Fungi, some of which are large, and all are important, were mainly published by the American Philosophical Society, and by the Linnean Society of London. Several of them are the joint productions of Dr. Curtis and of the able English mycologist Mr. Berkeley.

His other published writings mainly are "A Commentary on the Natural History of Dr. Hawks's 'History of North Carolina.'"—a good specimen of his appreciation of exact research and of sharpness of wit wholly free from acerbity; two papers in "Silliman's Journal" on "New and Rare Plants of the Carolinas;" and the botanical portion of the "Geological and Natural History Survey of North Carolina," in two parts;—the first, a popular account of the trees and shrubs, issued in 1860; the other, a catalogue of all the plants of the State, in 1867. This includes the lower Cryptogamia, especially the Fungi, of which he enumerates almost 2,400 species, while the Phænogamous plants are less than 1,900. All our associate's work was marked by ability and conscientiousness. With a just appreciation both of the needs of the science and of what he could best do under the circumstances, when he had exhausted the limited field in Phænogamous Botany within his reach, he entered upon the inexhaustible ground of Mycology, which had been neglected in this country since the time of Schweinitz. In this difficult department he investigated and published a large number of new species, as well as determined the old ones, and amassed an ample collection, the preservation of which is most important, comprising as it does the specimens, drawings, and original notes which are to authenticate his work. By his unremitting and well-directed labors, filling the intervals of an honored and faithful professional life, he has richly earned the gratitude of the present and ensuing generations of botanists. Several years ago he prepared drawings of the edible Fungi of the country, with a view to making them better known in an accessible and popular publication; but he was unable to find a publisher. He was much impressed with their importance as a source of food. During the hardships of the Rebellion, he turned his knowledge of them to useful account for his family and neighborhood; and he declared that he could have supported a regiment upon excellent and delicious food which was wasting in the fields and woods around him.

Anton Francis Spring, Professor of Physiology in the University of Liége, Belgium, died, January 17, 1872, in the 59th year of his age. He was the author of the classical monograph of *Lycopodium* and *Selaginella*, which was published thirty years ago in the memoirs of the Royal Academy of Sciences at Brussels. We are not aware that he did any later botanical work.

Hugo Von Mohl, Professor of Botany in the University of Tübingen. The following notice is reprinted from the Proceedings of the American Academy for June, 1872, being a part of the Council's report:

Hugo von Mohl, the acknowledged chief of the vegetable anatomists of this generation, died on the first day of April last. He was born at Stuttgard, April 8, 1805, the youngest of four brothers who all became men of mark in political and scientific life; Julius the orientalist and Hugo the botanist being the most distinguished. The latter was educated at the Stuttgard Gymnasium

and Tübingen University, where he studied medicine as well as natural history and physics. His first publication, while a student, in the year 1827, was his Essay on the Structure and Coiling of Tendrils and Twiners, written in response to a prize-question offered by the Tübingen Medical Faculty. In it he divined the real nature of the movements which coiling stems and tendrils execute, as has recently been clearly made out. In the following year appeared his inaugural dissertation on the Pores of the Cellular Tissue of Plants, in which his later views and discoveries, respecting the structure, growth, and component parts of cells, as subsequently developed, are already foreshadowed. About this time his choice was made for a scientific rather than a medical career; and he went to Munich to prosecute more advantageously his favorite studies. Here the late Von Martius and Zuccarini were his botanical masters, and Agassiz, Karl Schimper, Braun, and Engelmann his fellow-students. Here he made those researches upon the anatomy of ferns, cycads, and especially of palms,—the latter a most important contribution to Martius's great work upon palms, the former also contributed to another work by Martius,—which first displayed his remarkable talents for histological investigation, to which his subsequent scientific life was mainly devoted. His merits were promptly recognized by a call to the Imperial Botanic Garden of St. Petersburg, as assistant to its director, Dr. Fischer, and to the chair of Physiology in the Academy of Berne. He accepted the latter in 1832, and occupied it until 1835. Then upon the death of Schübler, he returned to Tübingen, accepted the professorship of Botany in its High School, in which chair and in that of Tübingen University the rest of his life was passed. Invitations to more prominent and lucrative positions, as, for example, to the Botanical chair at Berlin University, when vacated by the death of the veteran Link, were unhesitatingly declined. Although he published numerous (about ninety) special papers or articles, most of them important and timely, and some of great pith and moment, he resolutely declined to bring out any general work. His *Mikrographie* (1846) and his "Principles of the Anatomy and Physiology of the Vegetable Cell" are his only writings which may claim to be such. The latter, an admirable and still invaluable treatise, appeared as an article in Rudolf Wagner's Cyclopædia of Physiology, but is best known to English readers in its separate form, in a translation made by the late Professor Henfrey, with the author's sanction, issued by Van Voorst in 1852. A year or two later it was for a time understood, to the great satisfaction of botanists, that Mohl had agreed to take a prominent part in the production of a general Manual of the Anatomy and Physiology of Plants; but his promise was soon withdrawn. For thirty years he was one of the editors of the *Botanische Zeitung;* but the editorial labor must have devolved mainly upon Schlechtendal and his successor, although occasional articles from Mohl's pen appeared as late as the year 1871. During that year his health became seriously impaired;

yet, as the new year advanced, apprehension disappeared. Upon Easter Monday he was apparently well, and so retired to nightly rest; in the morning he was found to have died in sleep.

Louis Alphonse de Brebisson, died at Falaise in Normandy, April 26, 1872, at the age of 74 years. Besides the Flora of his native province, which passed through four editions, and some good papers upon *Algæ*, he was distinguished for his collections and knowledge of *Diatomaceæ*.

Robert Wight, M.D., died at his residence, near Reading, England, May 26, 1872, at the age of 76 years. He was born in East Lothian, Scotland, educated at the Edinburgh High School, and professionally at Edinburgh University, where he took his medical degree in 1816. He went to India, the field of his botanical career and most useful administrative activity for forty years, in 1819. He was first assistant surgeon and afterward full surgeon of a native regiment in the E. India Company's service; but was soon transferred to the charge of the Botanic Garden at Madras, and finally to that of the important Cotton plantations at Coimbatoor. His earliest botanical contributions occupy a conspicuous place in Hooker's Botanical Miscellany, commencing in 1830, and in the continuations of that work under other names and firms. In 1834, after a temporary sojourn in his native city, appeared the first volume of a model flora, the *Prodromus Floræ Peninsulæ, Indiæ Orientalis*, by Dr. Wight and Mr. (afterwards Professor) Arnott, of which their successors in the field remarked, that it is the most able and valuable contribution to Indian botany which has ever appeared, and one which has few rivals in thewhole domain of botanical literature. Dr. Wight returned to India immediately after the publication of this initial volume of the work, which was never continued. In India, assisted by native artists whom he had trained, he brought out two quarto volumes of "*Illustrations of Indian Botany*," with 182 colored plates; his *Spicilegium Nielgherrense*, of similar character, and finally his *Icones Plantarum Indiæ Orientalis*, in 6 volumes, with 2101 uncolored lithographic plates, and elaborate analysis, of unequal merit, many of them truly excellent but all wonderful, under the circumstances of their production. When he returned to England, nearly twenty years ago, his productive season, as it proved, was nearly over. But he distributed his collections with a liberal hand, as indeed he had always done, and in spite of a failing health enjoyed in a serene and happy old age the quiet country residence to which he had retired.

George Francis Reuter, the Director of the Botanic Garden at Geneva, Switzerland, for many years the companion in botanical journeys and investigation of M. Boisser, and the curator of his vast herbarium, died on the 22d of May last, at the age of about 55 years. He elaborated the *Orobanchaceæ* for De Candolle's *Prodromus*, published a Catalogue of the plants of the vicinity of Geneva, an Essay on the Vegetation of Castile, &c.; and was a man greatly esteemed by all who knew him.

Andreas S. Œrsted, Professor of Botany in the University of Copenhagen, died September 3d, at the age of 56 years. He began as a zoölogist, but since his return from his explorations in Costa Rica (1846–48), and his appointment to the chair of Botany in 1860, he has been one of the most active of Scandinavian botanists, and has treated with ability a great variety of subjects. In the Fungi, it was his interesting discovery that *Rœstelia* is a state of *Podisoma*.

Arthur Gris, an *aide-naturalist* at the Museum of Natural History, Paris, and one of the best botanical *elèves* of that establishment, died on the 18th of August last, at the early age of 42. He was associated with Professor Brongniart and was joint-author with him of several papers, mostly on new plants of New Caledonia. His independent publications were more numerous and important. Most of them related to anatomical and morphological topics. His thesis for the doctorate in sciences, in 1857, was founded on his microscopical investigation upon chlorophyl, which he was naturally led to undertake from the fact that his father, then deceased, had discovered that a chlorosed, or rather *dechlorosed*, plant was made green and vigorous under the action of salts of iron. One of his latest and most elaborate publications was a memoir upon the pith of plants, proving that this is much more diverse in structure than was supposed, and that the differences to a good extent coincided with ordinal characters.

Frederik Welwitsch, M.D., died in London, on the 20th of October last, in the 69th year of his age. He was a native of Carinthia; was educated at Vienna; was commissioned by the Würtemburg Unio Itineraria to collect the plants of the Azores and Cape Verd Islands; but on reaching Lisbon and finding good employment there, he made Portugal the field of his investigations, until, in 1850, he was sent by the Portuguese government to explore the natural history of its possessions on the west coast of Africa. His exploration of Angola and Benguela was rewarded by the discovery of more highly curious plants, probably, than any other that has been undertaken since Australia was opened to botanists; among them, and strangest of all, the genus which commemorates the discoverer, *Welwitschia mirabilis*, which Dr. Hooker, who described and illustrated it, does "not hesitate to consider the most wonderful, in a botanical point of view, that has been brought to light during the present century." Perhaps the limitation in the latter clause of the sentence is needless. This inhabits a most arid waste. In another district, under almost opposite conditions, Welwitsch had the good fortune to find the only Cactaceous plant indigenous out of America, viz., *Rhipsalis Cassytha*, and in a lake a new and most remote habitat of our *Brasenia peltata!* In his *Sertum Angolense*, a splendid memoir published by the Linnæan Society, with 25 plates, some of his most interesting discoveries are described; but the still unpublished portions of his collections must furnish most important contributions to the *Flora of Tropical Africa*, now in progress under the

orders of the British Colonial department and the editorship of Prof. Oliver of Kew. It is to be hoped that they may be more fully available for this Flora than they have thus far been.

12. JOHN TORREY.—This great bereavement, which took place on the 10th of March, was announced in the last number of this Journal. For want of space the biographical notice is deferred to the June number. A. G.

13. *Sachs' Lehrbuch. 3d edition. Lehrbuch der Botanik nach dem gegenwärtigen-stand der Wissenschaft bearbeitet von* DR. JULIUS SACHS, Ord. Professor der Botanik in Würzburg. Dritte Auflage. Leipzig, 1873.—This treatise is divided into three books. The first is devoted to General Morphology of the Cell, Tissues, and Organs of Vegetation; the second to the subject of Special Morphology, and the elements of Systematic Botany. In the third, the author treats of Vegetable Physiology under the following heads: Molecular Force in Plants, Chemical Phenomena, General Conditions of Vegetable Life, Mechanics of Growth, Some phenomena of Motion, Sexuality, Origin of Forms.

The present edition has some important changes, the following subjects having been entirely re-written: *Hepaticæ*, *Lycopodiaceæ*, Action of Light, and Effect of Gravitation. Several other sections have received additions to the text, and the work has been brought down to the summer of 1872. It is a treatise of such importance that the English translation, now said to be in preparation, will be warmly welcomed. G. L. G.

14. *The Expression of the Emotions in Man and Animals;* by CHARLES DARWIN, M.A., F.R.S., etc. With photographic and other illustrations. 374 pp. 12mo. 1872. London: John Murray; New York, D. Appleton & Co.—Darwin here reviews the various facts with regard to the expressions of feelings and emotions in animals, gives explanations of their introduction and permanence, and enforces therewith his argument in support of the theory of natural selection. The work shows, like his other volumes, the laborious, faithful and deep thinking philosopher, whatever may be the final decision with regard to some of his deductions.

III. ASTRONOMY.

1. *On the variation in the diameter of the Sun;* by A. SECCHI. —Secchi has reduced a series of observations on the diameter of the sun, extending from July 12, 1871, to July 21, 1872, made by his assistant, P. Rosa. Chronographic transits of the sun were taken over 19 fixed threads and one movable thread, thus giving 20 transits for each limb of the sun. The probable error of an observation deduced from the 20 threads was $0''{\cdot}31$, and the minimum error was $0''{\cdot}5$ of arc. He thus obtained 187 reliable determinations, which were rendered comparable by having been first reduced to the mean solar distance. These observations corresponded to heliographic latitudes comprised between 0 and 26 degrees. While the probable error never exceeded $0''{\cdot}5$ of arc,

the isolated determinations often differed by 3, 4 or even 5 seconds. Secchi thinks that this difference cannot be attributed to accidental errors, for they existed during several consecutive days, and passed insensibly from one value to another; also, the comparison of these measures with analogous ones made at Palermo show variations sufficiently corresponding to those found at Rome, to prove the reality of these variations in the solar diameter.

From an examination of the curves of these variations, Secchi deduces that the diameter had minima values when the number of spots and protuberances were at the maxima. This result shows that there is a physical relation which merits a special study. The region between 20 and 23 degrees of heliographic latitudes affords the smallest diameters, and this is exactly the zone where the solar activity is the greatest, as resulted from the comparisons and extent of the protuberances and spots. Secchi thus provisionally accounts for the above relation. The border of the sun is not perfectly defined, the want of definition in its contour being, probably, due to the light of the chromosphere, which, very bright at its base, fuses to some extent with the photosphere. The ordinary diameter of the sun is composed of the diameter of the photosphere augmented by this brighter inferior layer, whose height equals 4 seconds, which his measurers assign as the difference between the semi-diameter observed and that of the ephemeridis. If this explanation be true, knowing that the chromosphere is sometimes more and sometimes less brilliant, it results that the solar diameter, thus augmented, will be found to diminish and to increase with the brilliancy of the chromosphere.—*Memorie della Societâ degli Spettroscopisti Italiani*, *Dispensa* 9ª, Sept. 1872. A. M. M.

IV. Miscellaneous Scientific Intelligence.

1. *The Tyndall Endowment.*—Prof. Tyndall, as is generally known, generously devoted the entire avails of the lectures he delivered in the United States to the cause of scientific training and for the advantage of scientific students. In his "Lectures on Light," just published by the Appletons, we have a statement (p. 190) of the money returns by his thirty-five lectures, as follows:

"Boston, six lectures, $1,500; Philadelphia, six lectures, $3,000; Baltimore, three lectures, $1,000; Washington, six lectures, $2,000; New York, six lectures, $8,500; Brooklyn, five lectures, $6,100; New Haven, two lectures, $1,000; making the total $23,100.

"Of this amount, the surplus above expenses, amounting to upward of $13,000, was conveyed by an article of trust to the charge of a committee, consisting of Prof. Joseph Henry, Gen. Hector Tyndall, and Prof. E. L. Youmans, who are authorized to expend the interest in aid of students who devote themselves to original researches."

Prof. Tyndall's first thought was to provide for the residence of such students at some German university, reserving to himself the choice of the institution, the candidate being selected by his Trustees. But he finally gave the Trustees a discretionary power

to decide upon the place of study for the candidate whether in America or Europe.

Prof. Tyndall also gave $250 to the Yale Scientific Club in aid of original research.

2. *The Depths of the Sea. An account of the General Results of the Dredging Cruises of H. M. SS. "Porcupine" and "Lightning" during the summers of* 1868, 1869, *and* 1870; by C. WYVILLE THOMSON. 8vo, 527 pages, with 84 cuts and 8 maps and diagrams. New York and London: Macmillan & Co., 1873.—In this work the author has given in popular form a very interesting and somewhat detailed account of the several deep-sea dredging expeditions sent out by the English government in 1868, 1869, and 1870, under the scientific direction of the author, Dr. Wm. B. Carpenter, and Mr. J. Gwyn Jeffreys.

The principal zoölogical, physical, and chemical results of the expeditions are presented clearly and in a very readable form. Many of the more interesting and remarkable of the deep-sea animals discovered during these expeditions are illustrated by excellent figures, and many are also described, for the first time, in this work.

A full account is given of the equipment of the vessels, "Lightning" and "Porcupine," detailed for these expeditions, together with descriptions of the apparatus used in sounding, dredging, etc. One chapter is devoted to a history of deep-sea soundings, with descriptions and figures of the various kinds of apparatus employed. In another chapter a historical sketch of scientific dredging is given, with special reference to the deep-sea dredgings undertaken within a few years past.*

In this chapter the author has, by an unaccountable mistake or oversight, failed to do justice to the U. S. Coast Survey and to Mr. Pourtalès, by whom the first dredgings beneath the waters of the Gulf Stream were undertaken in 1867 and carried on with great success during three seasons. The author says (p. 277), "In the year 1868 Count L. F. de Pourtalès, one of the officers employed in the United States Coast Survey under Professor Peirce, commenced a series of deep dredgings across the Gulf Stream off the coast of Florida; which were continued in the following year, and were productive of most valuable results." He also quotes an extract from a letter written by Mr. Pourtalès to one of the editors and published in this Journal, giving a very brief account of the results of the second expedition (1868). But he nowhere alludes in any way to the previous expedition, in 1867, which antedates the first English expedition an entire year, although Mr. Pourtalès had published the very interesting and important results of that exploration in December, 1867, in the Bulletin of the Museum of Comparative Zoölogy, No. 6, together with descriptions of many of the new and remarkable animals obtained. These discoveries and the paper referred to were extensively noticed and discussed at that time by the scientific periodicals,

* The writer has already given in this Journal (II, xlix, p. 129, 1870) a sketch of the deep-sea explorations up to that date.

both in this country and Europe, and were universally regarded as of the highest interest and value. Moreover, this pioneer expedition and the report upon it have been constantly referred to in all the subsequent publications upon the subject by Mr. Pourtalès, Mr. A. Agassiz, Mr. Theodore Lyman, and others, in the Catalogues and Bulletins of the Museum of Comparative Zoölogy and elsewhere.* The zoölogical results were also given in Günther's Zoölogical Record. It cannot, therefore, be supposed that Dr. Wyville Thompson and his colleagues were ignorant of this earliest exploration, or of its results. It has, moreover, been repeatedly referred to in English periodicals and in public addresses delivered and published in England.

Nevertheless it is in no way referred to; not even in the published correspondence between Dr. Thomson, Dr. Carpenter, General Sabine, Mr. Romaine, and the secretary of the Royal Society, which preceded the organization of the first English expedition, in 1868. Yet it was generally believed, at the time, that the success of the American exploration had some influence upon the fitting out of that expedition. The first of the published correspondence is a letter from Dr. Thomson to Dr. Carpenter, dated May 30th, 1868, reciting the desirability of such explorations, suggesting the possible results [already in part realized by Pourtalès], and alluding to the important discoveries made by Dr. G. O. Sars, off the Loffoden Islands, but not mentioning Pourtalès or the United States Coast Survey.

One very important chapter is devoted to the deep-sea temperatures, with descriptions of the instruments, etc. A chapter is also devoted to the Gulf Stream. In this the various opinions and theories of previous writers are given and discussed, in the light of the numerous facts recently acquired concerning its phenomena.

It will be of interest to many to learn that the author does not sanction the direct-heat theory recently proposed and strongly urged by his colleague, Dr. Carpenter, to account for this and other ocean currents and a supposed general oceanic circulation, due to the differences in temperature and density between the tropical and polar waters. Dr. Thomson adopts the generally received theory, that the winds, and especially the trade-winds, furnish the motive power for ocean currents.

The last chapter is devoted to a discussion of the "continuity of the chalk." In this place the author has so modified his former views (or at least the statement of them) concerning this point that very few geologists will be disposed to oppose them. As he now explains his theory, viz: that a portion of the Atlantic basin has been occupied by a deep-sea from the Cretaceous to the present time, and that in this basin a chalk deposit has been continually forming ever since the Cretaceous period, or even earlier,

* A brief historical sketch of the Gulf Stream explorations is given by Pourtalès in the introduction to his "Deep Sea Corals," Illustrated Catalogue of the Museum of Comparative Zoölogy, No. IV, 1871; and in Petermann's Geographische Mittheilungen, Heft xi, 1870.

under similar conditions, it is unobjectionable. But it is no more logical to say, on this account, that we may be regarded as "still living in the Cretaceous period," than it would be to say we are still living in the *Glacial period*, because glaciers still remain in certain localities, and have persisted since the Glacial period. That the abyssal fauna of the Atlantic has many relations with the ancient Cretaceous fauna of the same region is certain, from the researches of all the naturalists who have described the deep-sea forms; and in this country, Mr. Pourtalès and Mr. A. Agassiz have brought out these relations very strongly in respect to the corals and Echini, but these relations are only *generic*. No undoubtedly identical *species* have been discovered, unless among the Foraminifera.

When we take into consideration the *totality of life*, both of the land and waters, in the Cretaceous and the present periods, the difference is enormously great, and the resemblances very few and exceptional. Many of the animals of Australia have very close relations with those of past geological periods, or in other words Tertiary and earlier types have persisted there, while in other regions they have been exterminated, or supplanted by other groups. Are we, on this account, to regard the Australian fauna as belonging to the Tertiary, Cretaceous, or Jurassic periods? Such instances of the persistence of certain ancient genera and families of animals and plants, in particular countries or under peculiar circumstances, are very numerous, and many of those that have long been known are quite as remarkable as those now brought to light from the deep-sea, but many of them have become too familiar to attract the attention that they deserve. A. E. V.

3. *The Challenger Expedition.* Letter from Prof. Wyville Thompson. (Nature, March 20.)—H. M. S. *Challenger* cast off from the jetty at Portsmouth, at 11.30 A. M. on December 21, with a low barometer. A strong southwesterly breeze was blowing, and the drum up; so that, especially in a season like the present, the prospect was not promising for the first few weeks of her voyage round the world.

The result justified the drum, and for a week we were knocking about the mouth of the Channel and the Bay of Biscay, making slow progress southward. It was perhaps as well to get a good shaking at first. It showed at once where there was a screw loose, and gave a chance to tighten it up. A sharp cyclone which caught the ship on her way from Sheerness to Portsmouth had already tested pretty fully the stowing of the apparatus, and although the *Challenger* rolls considerably when she is put to it, (over 35°), not a single instrument shifted, and not a glass was broken, either in the zoölogical workroom, or in the chemical laboratory. Just before we got to Lisbon the weather improved a little, and we got some soundings and took one or two trial hauls with the dredge.

After leaving Lisbon on January 12 the wind was again fresh, but between Lisbon and Gibraltar we made some important experiments, and found, among other things, that we could work

easily and successfully with the common trawl down to 600 fathoms. I am now writing about 100 miles north of Madeira, and since leaving Gibraltar the weather, though at first breezy, has been on the whole fine. We have taken several successful navigative sounds at great depths, and we have trawled successfully at 2,125 fathoms, and recovered many interesting animal forms, several of them new to science, and others of extreme rarity and beauty. Still we must regard our work up to the present time as only tentative. The weather has been against us. It is altogether a new experiment to dredge from so large a ship, and it seems to present some special difficulties, or at all events to require some management. The weight of the ship is so great that there can be no "give and take" between it and the dredge, such as we have in the case of a smaller vessel. If there is any way on, the impulse to the dredge is irresistible, and seems to tend to jerk it off the ground. This difficulty can no doubt be met, but the only way of meeting it appears to be by using a length of rope greatly in excess of the depth—and having weights. A single dredging operation may thus occupy a great length of time, but in compensation we have the greater size and efficiency of this dredge. The few trials which we have already made have been all in the direction of improvement, and I have little doubt that under Captain Nares' skillful management what little difficulty is still felt will shortly disappear.

The *Challenger* is a spare-decked corvette of 2,000 tons displacement. This particular build gives her an immense advantage for her present purposes, as she has all the accommodation of a frigate, with the handiness and draught of water of a corvette. Sixteen of the eighteen 68-pounders which form the armament of the *Challenger* have been removed, and the main-deck is almost entirely set aside for the scientific work. The after-cabin is divided into two by a bulk-head, and two little rooms thus formed—still gay with mirrors, and pictures, and new chintz, and bright with home faces—are allotted to Captain Nares and myself. The fore cabin, a handsome room, 30 feet long by 12 feet wide, into which these private cabins open, the captain and I use as a sitting room, the port-end with its writing table and work table, and its book-cases packed with old home favorites, being appropriated to my use and that of my secretary, Mr. Wild; while the captain has arrangements at the starboard end of the same kind. Two sets of cabins have been especially built on the after-part of the main deck for this difficult part of the scientific work. On the port side a commodious zoölogical work-room is occupied by the naturalist of the civilian staff, while the chart room corresponds with it on the opposite side. Toward the middle of the main deck, on the port side, there is a dark room and a working room for the photographer, and on the starboard side Mr. Buchanan has his chemical and physical laboratory.

Nearly the whole of the fore-part of the main deck is occupied by the dredging and sounding gear, Mr. Seimens's photometric and

thermometric apparatus, and the more cumbrous of our machines, such as the hydraulic pump, the aquarium, and other valuable articles, of which a detailed description will be given hereafter.
* * * * *

Dredging and sounding are carried on from the main-yard. A strong pennant is attached by a hook to the cap, and then by a tackle to the end of this yard. A compound arrangement of fifty-five of "Hodges' accumulators" is hung to the pennant, and beneath it a block through which the dredge rope passes. This arrangement appears to answer better than the old one of dredging from a derrick.

For the first two or three hauls in very deep water off the coast of Portugal, the dredge came up filled with the usual "Atlantic ooze," tenacious and uniform throughout, and the work of hours in sifting gave the very smallest possible result. We were extremely anxious to get some idea of the general character of the fauna, and particularly of the distribution of the higher groups; and after various suggestions for modifications of the dredge it was proposed to try the ordinary trawl. We had a compact trawl with a 15 ft. beam on board, and we sent it down off Cape St. Vincent at a depth of 600 fathoms. The experiment looked hazardous, but to our great satisfaction this trawl came up all right, and contained, with many of the larger invertebrata, several fishes. Two of these belonged to the genus *Macrourus*, and another of large size was unknown to us, approaching in many respects the genus *Mugil*. All the fishes were in a peculiar condition from the expansion of the air contained in their bodies. On their relief from the extreme pressure, their eyes especially had a singular appearance, protruding like great globes from their heads.

After this first attempt we tried the trawl several times at depths of 1,090, 1,525, and finally 2,125 fathoms, and always with success.

Several fishes, most of them allied to *Macrourus*, were added to the list. Several decapod crustaceans, and among the lower crustaceans, at 1,090 fathoms, a gigantic amphipod, of the family Hyperina, allied to *Phronima*. The eyes of this creature are very remarkable, extending in two great facetted lobes over the whole of the anterior part of the cephalothorax, like the eyes of *Aeglina* among Trilobites. This crustacean, which is three and a half inches in length, makes a splendid drawing, and reminds one of the old Eurypterids, is in process of description at the hands of Dr. von Willemoes Suhm.

Mollusca are very scarce in deep water, and our catches have hitherto been chiefly confined to such things as the species of *Nucula*, *Leda*, *Verticordia*, &c., familiar through the deep dredgings of the Porcupine. Among the molluscoids a haul in 1,525 fathoms gave us a lovely thing, a bryozoan forming, out of branches closely resembling those of *Acamarchis neritina*, a graceful cup, the bases of the branches united by a transparent stem between two and three inches high, like the barrel of a quill, or the stem

of a claret glass. This genus, which presents a general character totally different from anything hitherto known among recent Bryozoa, I mean to dedicate to Captain Nares, as an early recognition of the confidence and esteem which he has already fully gained from the scientific staff. *Naresia cyathus* certainly re-calls, in the most singular way, the Cambrian *Dictyonema*, a form which I had, however, hitherto been inclined to refer to the Hydrozoa.

The Echinoderms have yielded some exceedingly interesting species to the trawl; among them several examples of the beautiful little urchins, of which one specimen was taken by Count Pourtales, in the Straits of Florida, and described by Alexander Agassiz under the name of *Salenia varispina*. It is undoubtedly a true *Salenia*, and to an advocate of the doctrine of the "continuity of the chalk," it is pleasant to see in the flesh this little beauty, which has hitherto been reckoned among the lost tribes.

Among the star-fishes two species of the genus *Hymenaster* have occurred, and the ophiurids are well represented chiefly by large examples of several species of the genus *Ophiomusium*.

All the hauls of the trawl, down to 2,125 fathoms, have yielded many specimens of a singular Holothurid, of which a description will shortly be published by Mr. Moseley. The animal is of a rich violet color. * * *

Sea-pens and Gorgoniæ have occurred frequently, always remarkable for their brilliant phosphorescence. Captain Maclear is giving special attention to this beautiful phenomenon. A *Mopsea*, which shone very brilliantly, gave a spectrum extending from the green well on into the red, while *Umbellularia* gave a very restricted spectrum sharply included between the lines *b* and D. Of this wonderfully rare sea-pen, we took with the trawl a very fine specimen, with a stem 3 ft. long, at a depth of 2,115 fathoms off Cape St. Vincent.

As usual in deep-sea work sponges preponderated, and the order has added several novelties, chiefly referable to the Ventriculite group, the Hexactinellidæ.

Some fine new species of *Aphrocallistes* came up along the coast of Portugal, and off St. Vincent; with many spicules and more or less mutilated examples of Hyalonema, two or three species in fair condition of a species of *Euplectella*, with spicules which I cannot distinguish from those of *Euplectella aspergillum* — the Venus flower-basket of the Philippines. The form of the two sponges is the same, but our own specimens are quite soft, the spicules not fused into a continuous siliceous network.

The physical and chemical observations will be fully detailed hereafter. The temperatures off the Coast of Portugal corresponded very closely with those taken in the *Porcupine* in 1870, and the *Shearwater* in 1871, below the first 100 fathoms, through which at this season the temperature is nearly uniform.

4. *New York Central Park.*— Second Annual Report of the Board of Commissioners. 253 pp. 8vo. New York. 1872.—The Central Park in New York is one of the public works of which

every American may well feel proud. For science, it has already accomplished something in its fine collection of trees and shrubs of North America and exotic, in its well ordered Meteorological Observatory, under the direction of Mr. Daniel Draper; its museum of Natural History and Menagerie and its noble engineering works. The chapter on "the disaster to trees," from the severe weather of March 13th–15th, 1872, will be read with interest as touching the possible extinction of certain species long regarded as most hardy, in localities where they have often withstood a greater fall of temperature, but never before such a remarkable coincidence of distinct agencies. A list of species and genera, of which 7,853 individuals were killed, in those two days, is given in this report.

The question of climatic change, and especially the clearing of land as affecting the rainfall, is discussed by Mr. Draper, in his report for 1872, by comparison of the New York observations with those of other Atlantic cities; and the conclusion is the same reached some years ago, by Prof. Loomis, in his classification of about ninety years observations, at New Haven, viz., that there has been no change in the average rainfall when long periods of time are compared, although the annual means may vary considerably. A further comparison is made of the number of days during which the Hudson river remains closed by ice, and it is shown that for the five decennial periods from 1817 to 1867, this river has remained closed for 92, 92, 94, 90 and 91 days respectively, of which the general mean is 91·142 days. The extremes were 136 days in 1842–43 and 47 days in 1841–42. The conclusion is, that during 50 years there has been no sensible change in the average climate of the Hudson river valley. Mr. Draper's meteorological report is illustrated with beautiful synoptic charts exhibiting to the eye these general facts, and also one for each of the twelve months of 1872. The latter show the movements of the barometer, thermometer and wind, for each day, in blue, red and green curves.

5. *Lists of Elevations in the portion of the United States west of the Mississippi River.* Collated and arranged by HENRY GANNETT. Miscellaneous publications, No. 1, of the U. S. Geol. Survey of the Territories, F. V. Hayden, Geologist-in-charge. Department of the Interior. 48 pp. 8vo, 1873.—These lists give the elevations of some thousands of places over the whole country west of the Mississippi, including the Rocky Mountain Region and the States and Territories to the west, making a pamphlet of great value to the geographer and very convenient for reference. The determinations that have been made along the routes of the various expeditions and geological surveys and the lines of railroad, about military posts, besides many from other sources, are here tabulated. A previous edition was issued in 1872 by Professor Cyrus Thomas, of the Survey, and is noticed in vol. iv, p. 246, of this Journal. This has been much enlarged by Mr. Gannett. It is proposed to issue an edition annually, with such additions and corrections as

may be obtained. The publication is one of the very valuable results of the Hayden Exploring Expedition.

6. *Recent discussions in Science, Philosophy and Morals;* by HERBERT SPENCER. New and enlarged edition. New York: D. Appleton & Company. 1873. 16mo, pp. 349.—This interesting volume contains the following thirteen essays: 1. Morals and moral sentiments; 2. Origin of animal worship; 3, The classification of the sciences; 4. Postscript—Replying to criticisms; 5. Reasons for dissenting from the philosophy of Comte; 6. Of laws in general, and the order of their discovery; 7. The genesis of science; 8. Specialized administration; 9. What is electricity? 10. The constitution of the sun; 11. The collective wisdom; 12. Political Fetichism; 13. Mr. Martineau on Evolution.

The first six of these essays have grown out of the discussions called up by the appearance of the seventh, "The genesis of science," printed some seventeen years since, in the author's "Illustratiens of universal progress." They deserve and will receive, as they have done already, the careful consideration of all thoughtful minds. Readers of "Nature" will remember the essay on electricity in which the author considers the reasons for regarding all electrical phenomena as due to some kind of molecular motion, a proposition which no physicist will controvert.

All of Mr. Spencer's writings are marked by originality and are well adapted to excite thoughtful discussion even when they do not command general assent. B. S.

7. *A set of Wind and Current Charts for the Pacific, Atlantic and Indian Oceans* has just been issued from the Hydrographic office of the Admiralty.

8. *Half-hour recreations in Popular Science;* DANA ESTES, editor, Boston. No. 4 of this Series is a chapter on Spectrum Analysis discoveries, showing its application in microscopical research, and to discoveries in the physical constitution and movements of the heavenly bodies, from the works of Schillen, Young, Roscoe, Lockyer, Huggins and others; No. 6 contains Dr. Carpenter's two excellent lectures on the Unconscious Action of the Brain, and Epidemic Delusions; No. 7, the Geology of the Stars, by Prof. A. Winchell.

9. *Erratum in Sang's Logarithms.*—Mr. N. Willey sends us the following correction to Sang's tables, viz: page 131, for the logarithm of 82885 read 9184759, instead of 9185759.

OBITUARY.

BARON LIEBIG.—The eminent chemist, Baron Justus Liebig, died at Munich on the 10th of April, aged seventy years.

APPENDIX.

Notice of New Tertiary Mammals; by O. C. MARSH.

IN addition to the extinct Mammals already described by the writer, the Museum of Yale College contains some interesting remains of this group from the various Tertiary deposits of the Rocky Mountain region. Not a few of these specimens are new to science, and some of the more important are here described.

Orohippus agilis, sp. nov.

Additional specimens of this genus fully justify its separation from *Anchitherium*, and likewise show that it holds a most interesting intermediate position between that genus and the less specialized mammals of the *Palæotherium* type. The genus differs essentially from *Anchitherium* in having four functional digits in the manus, in having the first premolar nearly as large as the second, and in the absence of an antorbital fossa. The skull is elongated, and equine in its proportions. The orbit was not enclosed behind. There were three upper true molars, and four premolars. The radius and ulna were separate, and the latter bone is stouter than in *Anchitherium*.

The present species differs from *Orohippus pumilus* Marsh,* in having the inner cones of the upper molars more nearly of equal size, and each with a distinct basal ridge. The remains preserved indicate, moreover, a somewhat larger animal, which nearly equalled a fox in size.

Measurements.

Space occupied by upper molar series,	49·	mm.
Space occupied by upper true molars,	23·	
Antero-posterior diameter of penultimate upper molar,	8·	
Transverse diameter,	9·5	
Transverse diameter of distal end of humerus,	20·	
Transverse diameter of proximal end of radius,	17·	
Transverse diameter of distal end on articulation,	12·4	
Transverse diameter of distal end of ulna,	5·6	
Length of third metacarpal,	55·5	

The known remains of this species are all from the Eocene of Wyoming.

Colonoceras agrestis, gen. et sp. nov.

In its cranial characters and dentition, this genus resembles most nearly *Hyrachyus* Leidy, and *Helaletes* Marsh. It differs

* This Journal, vol. iv, p. 207, Sept., 1872.

especially from these genera, so far as they are known, in the presence of a pair of dermal horns on the nasal bones, which were strengthened to support them. These horns were placed opposite each other, and their position, in a nearly perfect skull in the Yale Museum, is indicated by two rugosities, which have their surfaces marked by radiating lines. In the present species, which was about as large as a sheep, the horns were widely divergent.

Measurements.

Space occupied by seven teeth in upper molar series,	77·	mm.
Extent of three true molars,	41·	
Distance between orbits,	62·	
Distance between apices of horn rugosities,	27·	
Length of frontals on median suture,	62·	
Expanse of occipital condyles,	40·	

The remains of this species at present known are from the Eocene of Wyoming.

Dinoceras lucaris, sp. nov.

This genus may be distinguished from *Tinoceras* Marsh (*Eobasileus* Cope), by the anterior position of the maxillary horns, by the elevated parietal crests, by the short and arched diastema, and by the compressed and trenchant canine tusks. From *Uintatherium* Leidy, so far as that genus is at present known, *Dinoceras* differs in the position of the occipital condyles, in the more anterior position of the posterior horns, and in the last upper molar, which lacks the external cone between the two transverse ridges, and has a second smaller tubercle behind the posterior ridge.

The present species, which may provisionally be referred to *Dinoceras*, differs from *D. mirabilis* Marsh,* aside from its larger size, in the structure of the upper molars. The penultimate has the inner posterior tubercle double, and the last true molar has a tubercle in the angle of the transverse crests, and also lacks the second posterior tubercle. The basal ridge is continuous on the inner side of each of the three upper premolars.

Measurements.

Space occupied by upper molar series,	157·	mm.
Extent of last three upper molars,	93·	
Antero-posterior diameter of last upper molar,	35·5	
Transverse diameter through posterior crest,	38·	

The locality and geological horizon of these remains are essentially the same as those of the preceding species.

* This Journal, vol. iv, p. 343, and vol. v, p. 117.

Oreodon occidentalis, sp. nov.

An interesting species of *Oreodon* occurs in the Miocene of Oregon, in the same deposits with *O. superbus* Leidy. It resembles *Oreodon Culbertsoni* in most of its cranial characters, but differs materially in the large auditory bulla, which is several times the size of the postglenoid process. The species is smaller than *O. major*, and has the frontals between the orbits more depressed, and the antorbital fossa deeper.

In comparing the various species of *Oreodon* some new points in the structure of this genus were observed. The dentition of all is essentially the same, the formula being as follows:—Incisors $\frac{3}{3}$, canines $\frac{1}{1}$, premolars $\frac{4}{4}$, molars $\frac{3}{3} \times 2 = 44$. The caniniform tooth of the lower jaw is clearly the first premolar, as Dr. Gill has stated. The metacarpals are slender, and those in *O. Culbertsoni* are about twice as long as those in *Dicotyles torquatus*. The first is wanting. The third and fourth are nearly equal in size, and had their coadapted faces immovably united by cartilage. The second and fifth are both well developed. The navicular and cuboid bones were loosely coössified, or separate. The phalanges are much more slender than in the Peccaries.

The following are some of the dimensions of a large specimen of *Oreodon occidentalis*.

Measurements.

Space occupied by last three upper molars,	45·5 mm.
Antero-posterior diameter of last upper molar,	15·5
Extent of last three lower molars,	53·
Distance between outer faces of postglenoid processes,	77·
Length of frontals on median suture,	54·
Vertical diameter of auditory bulla,	22·

The type specimen of this species was presented to the Museum of Yale College by Rev. Thomas Condon, who has done so much for the palæontology of Oregon. Other specimens were collected by the Yale party in the autumn of 1871.

Rhinoceros annectens, sp. nov.

There are two well-marked species of *Rhinoceros* represented in the Yale collections from the Miocene of Oregon. One of these Dr. Leidy has called *R. pacificus*;* the other appears to be undescribed. It was apparently about half the bulk of the former species, which it resembles in some of its dental characters. In the upper molars, however, the transverse crests approach each other much more nearly, and in the true molars preserved they are united, thus dividing the interposed valley.

* Proceedings Philadelphia Academy, 1872, p. 248.

The basal ridge, also, is much less developed on the inner side of the upper molars. Upper incisors were present, and one of the lateral ones was greatly compressed, and its crown very short, as in the existing *R. Javanicus.*

Measurements.

Antero-posterior diameter of penultimate upper molar,	27·	mm.
Transverse diameter,	36·	
Antero-posterior diameter of first upper true molar,	26·	
Transverse diameter,	34·5	
Antero-posterior diameter of first upper premolar,	20·6	
Transverse diameter,	17·	
Antero-posterior diameter of upper incisor,	21·	
Transverse diameter,	7·6	

The remains on which this species is mainly based were found, in November, 1871, in the John Day Valley, Oregon, by Mr. G. G. Lobdell of the Yale party.

Rhinoceros Oregonensis, sp. nov.

A second new species of this genus, much larger than either of the Miocene species, is indicated by portions of several individuals which were found by the Yale party, in 1871, in the Pliocene deposits of Oregon. One of these specimens is a penultimate upper molar which is quite characteristic, and differs widely from the corresponding tooth in any of the known species. At the union of the transverse posterior ridge with the outer cusp, there is a deep cavity, nearly circular, and enclosed by a vertical cylinder of enamel. The anterior crest, also, is divided, a strong branch being sent inward and backward from the posterior side into the main transverse valley.

Measurements.

Antero-posterior diameter of penultimate upper molar,	41·	mm.
Transverse diameter (approximate),	48·	
Transverse diameter of circular cavity,	6·5	
Distance from center of cavity to front margin of tooth,	21·	

This species appears to have been about two thirds the size of *R. crassus* Leidy, from essentially the same geological horizon.

Yale College, New Haven, April 24th, 1873.